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THE PSYCHOLOGICAL REVIEW

THE NATURE OF MENTAL PROCESS

BY HARVEY CARR

University of Chicago

This paper proposes the somewhat unorthodox view that the mental functions with which psychology concerns itself are in reality psychophysical, and at times neural, activities and that psychology shall study and attempt to comprehend these functions in their entirety. The author adopted this conception of the nature of mental process several years ago and is convinced from his teaching experience that such a mode of treatment possesses certain distinct advantages.

The conception may be contrasted with the more orthodox 'subjective' view which postulates psychophysical parallelism but which confines its efforts exclusively to a comprehension of the conscious or subjective aspect of these psychophysical events. Such a psychology studies color, sound, taste, and pain as experiential results but contends that the neural correlates of these sensory experiences belong to the domain of physiology. An emotion as a psychological phenomenon is described and defined in terms of sensational and affective processes subjectively regarded; the neural events involved in an emotion are relegated to the domain of physiology. The acts of memory, imagination, reasoning and will in so far as psychology is concerned with them consist merely of those aspects which can be immediately experienced; the neural events involved in the acts are quite important but their consideration involves a trespass into domains rightfully belonging to another science. Psychology thus deals exclusively with the purely conscious or psychic as opposed to the nervous and material. Psychology is differentiated

from the material sciences in virtue of a peculiar subject matter and a peculiar method of apprehending its data.

Our conception also accepts psychophysical parallelism as a working hypothesis; it contends that psychology shall study psychophysical processes in their entirety, and that it shall include within its domain activities which lie outside the field of consciousness. Psychology will study emotions, and acts of reasoning, memory and will, but it will define and envisage these acts as psychophysical processes, and attempt to comprehend the neural events involved as well as those aspects immediately experienced.

The conception allows a division of the field of organic functions between the sciences of biology, physiology and psychology along natural lines of cleavage based upon differences of interest, training and technical procedure. Irrespective of definitions, psychology has been concerned with an ultimate comprehension of those operations by which an organism in virtue of its previous experience is enabled to adapt itself to a complex and variable environment. Physiology and biology have been interested in other types of functional activity—different in character, evolutionary history, and biological significance. The three sciences represent distinctions which appeal to radically different types of human motive and scientific interest, and which necessitate different sorts of training and technical equipment. This paper is not concerned with the formulation of an exact definition delimiting the boundaries of the three sciences.

The conception is unorthodox only in relation to prevailing *definitions* of psychology. To my mind it is essentially in harmony with the dominant point of view of the science, and it is not wholly inconsistent with much of current practice. Sciences have a way of developing and outgrowing their definitions. Practice and attainment often fail to square with theory and definition. The subjective conception of the nature of mental process (mental as opposed to material) originated from philosophical interests at a time when a dualistic conception of the human organism prevailed. At present the prevailing point of view in the science is biological,

—a view which emphasizes the essentially unitary character of the human organism. The science is interested in certain modes of adjustment, and any adequate understanding of these processes of adjustment must necessitate a comprehension of the acts in their entirety. A division of any act of adjustment into its material and conscious aspects with the consequent treatment of but one component certainly gives a very inadequate comprehension of the phenomenon in question, and introduces a distinction which is not only without value, but which is likely to involve the student in many distracting perplexities. Our proposition, however, will necessitate no radical changes in much of current modes of procedure. As a matter of fact, many psychologies treat mental operations as psychophysical processes with only occasional lapses into a consistency with their subjective definitions of the science. It is our definitions that need revision, a revision in harmony with current tendencies and ideals.

The conception allows of a matter of fact treatment of the cause and effect relations in mental activity. One can assert that behavior is influenced by previous acts of memory or will and mean exactly what we say and what the unsophisticated mind understands by the statement. Psychologists will not be compelled to add for the benefit of the sophisticated qualifying phrases to the effect that although they asserted a causal influence of a mental act, yet they really did not mean it, but were forced to employ such statements by certain inadequacies of language. Any conception which allows of a natural and matter of fact treatment of the causal category in mental operations is at least deserving of respect. The new definition of the mental will permit a restatement and a solution of the mind-body problem more in accordance with common sense. Interactionism is logically possible; in accordance with popular belief we may say that our mind is influenced by bodily conditions and that our mind is also an effective influence upon bodily activities, for mind and body have been so conceived and defined in relation to each other that such statements are no longer logically or factually impossible. With our definition the distinction of mind and body is merely a distinction of two systems of organic function.

Our definition will include within the domain of psychology the non-conscious components of mental life. I refer to such phenomena as retention, memory disintegration, conflict of impulses, Aufgabe, unconscious motivation, the concept of habit, and the wealth of subterranean activities brought to notice by the investigations of abnormal psychology. The subjective psychologies have assumed several attitudes toward these phenomena. Some are logically consistent with their presuppositions and attempt to ignore such intruders. Others admit the significance of these data for their purposes, but consistently remind their readers that after all these phenomena really belong to the domain of physiology. Others include these events within the domain of their science, but feel compelled by motives of consistency to impose upon these processes some sort of a 'conscious label.' Witness such terms as unconscious, subconscious, co-conscious, psychical dispositions, etc. These are not merely negative terms, equivalent to neural; they have a positive significance. Since these activities have a mental significance and since the mental must also be conscious, these acts must be conceived in such a way as to possess certain positive characteristics of conscious process. For my part, I prefer the remaining possible mode of procedure, viz., to revise my definition of the nature of mental process in such a way as to include such data.

The subjective conception of mental process as something immaterial constitutes an inadequate tool for the physician in his attempt to comprehend the nature of the mental, or functional disorders. Watson, in a recent article,[1] has asserted his inability to understand the medical concept of mental disease. He cites a case which was diagnosed as being 'purely mental,' and which was described and defined wholly in conscious terms. Watson gives the impression that the physician was of the opinion that this disorder could not in any manner be stated in neural terms, that it was a disorder exclusively on the conscious plane without neural counterpart. Such a conception of the nature of the so-called mental diseases is of course foreign to current psychological

[1] 'Behavior and the Concept of Mental Disease,' *J. of Phil., Psychol., &c.*, 1916, 13, 589.

doctrines, and I doubt very much that such a view is universally prevalent in the medical profession. I am willing to admit that this and similar crude and preposterous conceptions are to be met with, but, unlike Watson, I am inclined to place the blame for this unfortunate state of affairs upon psychology rather than upon medicine. Medicine has merely adopted current conceptions. Psychology must be held responsible for the fact that better and more adequate conceptions were not available. Given a conception of mind as something non-nervous and non-material, crude notions of the nature of a mental disease must result. The mental will be distinguished from other diseases in terms of immaterial vs. material instead of functional *vs.* organic. The conception needlessly introduces into the discussion of mental disease and its treatment the old philosophical question of the relation of mind and body stated again in terms of the conscious *vs.* the physiological, or neural. Certainly the injection of such philosophical questions in discussions of the treatment of mental disease adds nothing of any positive value, introduces a perplexing distraction, and to my mind is wholly unnecessary. The old philosophical problem vanishes at once if we start out with the assumption that the disordered mental functions are in reality psychophysical events.

The psychophysical conception of mental process offers a mediating point of contact for the two extremes of subjectivism and behaviorism. Such a view permits the widest latitude as to methods of approach; it permits mental processes to be studied from the standpoint of immediate experience, of objective observation, or of clinical data. This suggestion of a common ground for the subjectivists and the objectivists, I am well aware, will evoke no approval from either of the warring camps. This phase of the argument is designed exclusively for the benefit of the neutrals. Our program will differ from that of the subjectivists in allowing an objective mode of approach to the problems of psychology; it will differ from behaviorism in two respects: it admits that the study of conscious data has given us much useful knowledge of the nature of mental operations and that further progress is

possible in the future. Behaviorism as defined logically includes the whole field of organic function. Psychology should be content with a more modest program and make a more reasonable division of the field of functional activity among the sciences of psychology, biology, and physiology.

The conception will modify our attitude toward the purposes and methods of comparative psychology. The subjectivist to be consistent must define the object of comparative psychology as a reconstruction of the inner life of an animal. Such a program appeals to some minds; to other minds it is repellent. The latter are impressed with the insuperable difficulties and limitations of the interpretative process, and are inclined to regard any results achieved as somewhat futile even if logically valid. Our conception makes possible for those who prefer it a purely objective or behavioristic science of comparative psychology, and yet permits others to reconstruct the conscious life of organisms if they so desire.

An exclusively subjective psychology is prone to meet certain needless difficulties in its presentation. The psychological discussion of a mental event is usually followed by an explanation in terms of neural mechanisms which are labelled physiological. The psychological process viewed by itself impresses the mind of the student as a peculiarly inert and unimportant thing. When the neural mechanism is now added, the process takes on life and significance; mental processes can now do things and be effective instruments of organic adjustment. Moreover, the neural chain of events is, by hypothesis, more complete than the conscious aspect for not all neural events are represented in consciousness. It is small wonder that many students decide that they must look to physiology for any complete and real explanation of mental life. This sceptical attitude of the student toward psychology is not wholly unjustified, nor is it confined entirely to immature students; as a matter of fact this attitude toward our science is quite prevalent among the physiologists. I do not contend that it is impossible to meet such a position in a satisfactory manner; the significant point is that this attitude of scepticism is wholly unnecessary and exceedingly

detrimental. No science can afford to be placed in a defensive and apologetic position. The dual presentation further raises such distracting questions as the relative values of the neural and conscious components in an act of adjustment, their causal interrelations, and the necessity for a double treatment of mental events. All these are valid problems but they are philosophical in character and have no place in an empirical, matter of fact, science. Psychology generally recognizes the philosophical character of these questions and their tendency to distract the mind of the student from the main task at hand, and the usual method of attempting to ignore them is by the adoption of parallelism as a working principle. To my mind this method fails utterly to achieve its purpose. Such philosophical questions must necessarily obtrude with a dualistic mode of presentation; in fact this method was developed at a time when psychology was studied primarily as an introduction to philosophical problems, and a better method for this purpose would be hard to devise. These difficulties are at least minimized if not eliminated by adopting the conception of this paper. Parallelism is adopted as a working hypothesis in a matter of fact way without calling the student's attention to it. The total activity is made the object of study; the dichotomy involved is not one of process, but one of method of approach or apprehension.

In conclusion we may say that there are no fixed and immutable boundary lines between sciences. Any science includes within its domain whatever is pertinent to its primary interest. If mental acts are means of organic adjustment, then these acts must be conceived and studied as such devices. If neural events are essential parts of the act, the conception of the mental must be broadened so as to include them. There is no a priori necessity for defining the mental in nonmaterial terms, except habit and the force of tradition. The concept of the mental must be adapted to further the primary task of psychology as it is now conceived.

A REFORMULATION OF THE LAW OF ASSOCIATION

BY WALTER S. HUNTER

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I

The reformulation that this paper undertakes is to render explicit certain facts and points of view that the writer finds implicit in current psychological thinking. The 'law of association' as formulated and discussed by British psychologists concerned itself with ideas and sequences of thoughts and not with sequences of conscious states in general (which would have included sequences of sensory data). Such was the case with Aristotle also and with the intervening writers. Observation was warped primarily by the prevailing interests in logic and epistemology and by the tendency to overestimate the importance of vision in consciousness.

Aristotle says, *e. g.*: "Hence, when we are recollecting we keep stimulating certain earlier experiences until we have stimulated one which the one in question is wont to succeed. And just so we hunt through the sequence, thinking along from the present or some other [thought], and from similar or contrasted or contiguous."[1]

In Berkeley's writings much use is made of association, or suggestion as he terms it. And it is particularly to be noted that sequences of sensations as opposed to sequences of ideas are produced by the Deity. In David Hartley there is a thoroughgoing physiological as opposed to a logical associationism. Hartley's classical formulation is as follows: "Any sensations *A*, *B*, *C*, etc., by being associated with one another a sufficient number of times, get such a power over the corresponding ideas *a*, *b*, *c*, etc., that any one of the sensations *A*,

[1] Quoted from H. C. Warren, 'Mental Association from Plato to Hume,' PSYCHOL. REV., 1916, 23, p. 210.

when impressed alone, shall be able to excite in the mind *b*, *c*, etc., the ideas of the rest."[1] Hartley's physiological interests led him to apply the law specifically and in detail to habit formation. Obviously here the cases described—particularly the speech habit (see below),—must furnish data in harmony with our present contention; but Hartley never realized it. Had he or other writers done so, they must have recast their general law.

The point of view of these writers and even their formulations of the law have persisted into current writing in spite of the fact that a constantly increasing growth in the knowledge of habit and thought has been making the conventional law inadequate. The following quotations from current textbooks will indicate the present general attitude.

In Calkins[2] we find: "Successive association is the sequence of an imagination on a perception (or another imagination), a sequence which is attributed (in after-reflection) to the previous occurrence, simultaneously or in swift succession, of the two experiences." This is repeated in the table on page 359.

Titchener[3] says: "We then find this: that whenever a sensory or imaginal process occurs in consciousness, there are likely to appear with it (*of course in imaginal terms*)[4] all those sensory and imaginal processes which occurred together with it in the earlier conscious present. This we may term the law of association."

Angell[5] formulates the matter as follows: "The law of association asserts that whenever two images, or ideas, have been at any time juxtaposed in the mind, there is a tendency, if the first of them recurs, for the other to come with it." Judd and Thorndike state the law in a very general form which could be interpreted to cover our present point; but the context in each case indicates clearly that it is the old view of the

[1] Priestly, Jos., 'Hartley's Theory of the Human Mind.' London, 1775, p. 14, Prop. 5.

[2] Calkins, M. W., 'First Book in Psychology,' 4th Ed., 1914, p. 116.

[3] Titchener, E. B., 'Textbook of Psychology,' 1910, p. 378.

[4] Italics mine.

[5] Angell, Jas. R., 'Psychology,' 4th ed., 1908, p. 206.

nature of the second term of the association which is in their minds. The present thesis receives no consideration.

Since the Greeks it has been recognized that the principle of association is the principle of habit formation. But the development and interrelations of the two thoughts has ended there because of the interests above mentioned. At the present, behavior studies and the critical literature on thought afford us the ground for and even require a recasting of the doctrine of association. The revised version needs, I believe, but to be stated in order to carry conviction in many minds: *If A and B are experienced together in space or time and if later one is experienced either in sensory or in imaginal form, it tends to arouse the other either in sensory or in imaginal form.* In other words the second member of an association may be and often, if not usually, is a sensory process. By the term sensory process I would include both sensation and perception. The essential point is that the conscious state which forms the second term of the association is often conditioned by a present and on-going peripheral (sensory) activity. It is passing strange that this has not been explicitly stated and systematically incorporated before now. Ever since Ebbinghaus's tests on nonsense syllables 'associations' have been studied which have involved the reproduction or recall of sensory material (auditory-vocal-motor). Even sub-liminal associations have been tested by the saving method and otherwise with the constantly present purpose of making possible the recall in sensory form of certain material. The present formulation does not attempt to explain the fact of association, nor does it attempt to analyze the factors involved in the formation of associations (habits). It proceeds on the assumption that fundamentally 'associationism' cannot be outgrown and left behind any more than neural habits and the setting of synaptic connections can be so treated. It is therefore important that the 'law' be made adequate and general.

II

When psychologists from Hartley to the present time have attempted to sketch the neural side of the 'law of association'

they have talked in brain terms and not in terms of the nervous system as a whole. This has followed by virtue of the assumption that the second term of an association must be an image (or a centrally aroused process). Hear James[1] on the neural side: "I shall try to show, in the pages which immediately follow, that there is no other *elementary* causal law of association than the law of neural habit. All the *materials* of our thought are due to the way in which one elementary process of the cerebral hemispheres tends to excite whatever other elementary process it may have excited at some former time. The number of elementary processes at work, however, and the nature of those which at any time are fully effective in rousing the others, determine the character of the total brain action, and, as a consequence of this, they determine the object thought of at the time. According as this resultant object is one thing or another, we call it a product of association by contiguity or of association by similarity, or contrast, or whatever other sorts we may have recognized as ultimate." "Let us then assume as the *basis* of all our subsequent reasoning this law: *When two elementary brain-processes have been active together, or in immediate succession, one of them, on reoccurring, tends to propagate its excitement into the other.*" In the last sentence James indicates that the 'law of neural habit,' essential for association to his mind, is one dealing with central processes.

All that this does is to read over into neural terms the faulty formulation of the law as indicated above. Introspection can at the best only note the sequences of conscious states. If these can be traced to a prior contiguity, they are cases of association. On the basis of the introspective report on the nature of the associated contents, one can then proceed to formulate the neural changes involved. The following illustrations make clear that such neural changes involve far more than the cortex,—that we are dealing with peripherally and not with centrally aroused processes: I see (*A*) and touch (*B*) an object which results in my drawing back (*C*), trembling (*D*), screaming (*E*), or swearing (*F*). Later I see

[1] James, Wm., 'Principles of Psychology, 1890, vol. I., p. 566.

(*A*) or touch (*B*) the object (or do both) and at once *C*, *D*, *E*, or *F* appear. And they may and do appear in sensory form. Here belong the so-called cases of emotional and affective memory which were so much discussed some years ago. I see (*A*) an object and it gets bound up with unpleasantness (*B*). Later when I have *A* or its image *a*, the unpleasantness returns as *B* in the concrete with all its organic and motor changes. Or again I see a word and hear it at the same time, later I see or think of the word and the sound is again produced by my vocal organs. It is hardly necessary to point out how in these cases the nervous impulses shuttle back and forth between the brain and the periphery of the body. Central arousal and images do not monopolize these situations. On the conscious side we have had a succession of sensations whose connection reflection can trace to a prior contiguity. This is what we mean by "mental" association and this is where the law starts or gets its basis. Neurally we have had widespread activity unconfined to the hemispheres.

Current writing is emphasizing the relation of language and the vocal processes to thought. It seems clearly established that thinking can go on without detectable imagery. The 'meanings' to most psychologists (and I agree) are not 'pure thoughts,' but accompany, among other things, the kinæsthetic sensations (sensory processes) of the vocal organs. These language cases are functionally the most important instances where the second member or succeeding members of an associative train are sensations (as I should say), or peripherally initiated processes (as others would say).

It should not be objected that with the increasing automatism these peripheral processes become short-circuited and drop from consciousness. The same could be said of the imaginal members of the association possibly even more truly. It is doubtlessly the case that with both sorts of material the meaning is usually the clearest and most prominent feature; but that again is a point for any formulation of the law of association and not alone for the present one. The essential thing for us here is that the meaning can be carried, and is carried, as well by sensory as by imaginal proc-

esses and that by virtue of the internal control of their stimuli these sensory processes may constitute the second terms of an associative sequence.

Nor should it be objected that the present account fails to consider simultaneous association or persistent association, where one part of an associated complex persists over into the succeeding moment. The explanation of such cases need be no easier with images than with sensory processes. The play of muscles from moment to moment offers perhaps the clearest illustration of this type of event. Muscles 1–10, let us say, are active at a given moment and by their activity lead to the contraction of muscles 8–15. A common element, muscles 8–10, is found in each activity. If we speak from the conscious side, we have a core of kinæsthetic (and cutaneous) sensory experiences persisting through two successive moments of time. Practically this situation is always present and gives us our feeling of bodily presence and of the continuity of the self. Here again our account could be much more complete if we could formulate the causes of the setting of associations (synaptic connections). Such incompleteness, however, does not affect our essential point, nor is it a difficulty peculiar to the present thesis.

III

Sensory processes have their functional significances in the responses that they initiate. And so, stated the other way round, what adaptive movements shall take place depends very largely upon the sensory processes available. Certain of these sensory processes are practically beyond the organism's control. Certain others on the contrary are very much within the control of certain organisms and to a variable degree within the control of all. An animal's control of its own movements or responses is to a large extent dependent upon the sensory processes that it can itself initiate. Self-initiated stimuli in most animals are those of hearing, cutaneous sensitivity (pain excluded?), and kinæsthetic, organic and static sensitivity. Those that are not self-initiated are taste, smell, and vision. The last statement, of course, needs

much qualification. Many animals produce odors and others have phosphorescent organs which emit light. The same may be true also of taste, particularly with aquatic animals. In man the qualification that I feel it most necessary to urge is that of visual sensations produced by intention and probably initiated by the efferent optic fibers.

Wherever we find sequences of habits, there we should expect to find stimuli under the organism's control. Otherwise in order to have a habit sequence such as speech, running a maze, or solving the delayed reaction, it would be necessary that some non-controlable stimulus initiate each link in the response. But the contraction of one set of muscles stimulates receptors which arouse the succeeding muscular activity. If this sequence is essentially constant and if the receptors condition consciousness, we have a typical case where an associative sequence is sensory in its entirety. Writers on association at the time the law was fixed upon psychology were too fascinated by vision (and logic)—and vision is a sense whose stimulus (light) is practically non-producible by man's own activity. What more natural then than that in formulating a rule of sequences, they should have said that the second terms of associations could not be sensory but must be imaginal. Indeed the last sentence is misleading. The question was not clearly enough considered for them to use the terms 'could not be' this or 'must be' that. But this error, I believe, proceeded from tacit assumptions as I have indicated. And the same assumptions, tacitly made, still persist and lead to our conventional formulations, although such formulations are as wide of the mark as I have tried to indicate. It is true that where writers come to consider kinæsthetic habits, *e. g.*, they give essentially the account that I have sketched above. But at the same time they forget that so far as the conscious side is concerned they are dealing with genuine cases of the law of association; and when formulating that law, they forget the other situation. Nowhere do they bring the two into working union. Suggestive statements are alone to be found. I cite three instances:

First, Hartley's account[1] of the growth of the speech habit:

[1] Priestley, *op. cit.*, pp. 33–34.

"About the same time . . . the muscles of speech act occasionally in various combinations, according to the associations of the motory vibratiuncles with each other. Suppose now the muscles of speech to act in these combinations at the same time that sound is produced from some agreeable impression, a mere sensation, or a slight associated cause, which must be supposed to be often the case, since it is so observable that young children, when in a state of health and pleasure, exert a variety of actions at the same time. It is evident, that an articulate sound, or one approaching thereto, will sometimes be produced by this conjoint action of the trunk, larynx, tongue, and lips; and that both these articulate sounds, and inarticulate ones, will often recur, from the recurrence of the same accidental causes. After they have recurred a sufficient number of times, the impression which these sounds, articulate and inarticulate, make upon the ear, will become an associated circumstance (for the child always hears himself speak, at the same time that he exerts the action) sufficient to produce a repetition of them. And thus it is that children repeat the same sounds over and over again, for many successions, the impression of the last sound upon the ear exciting a fresh one, and so on, till the organs be tired."

Miss Calkins also comes near making the essential point when she points out that any state of consciousness plus a feeling of generality is a concept. (". . . when I think of fear, the consciousness of generality accompanies a genuine, though certainly faint, experience of that emotion."[1]) Now since thinking consists in a sequence of concepts, or their physiological equivalents, she should have added that it is possible to have a *sequence* of internally controlled but peripherally initiated states of consciousness. And this would have forced her to re-state the law of association whose phrasing I have quoted above.

James, too, came perilously near discovering the point that I am making. There is one sentence in the very chapter on Association that gives a beautiful illustration of the fact that the second term of an association may be sensory.

[1] *Op. cit.*, p. 147.

". . . probably every one who bathes himself in a certain fixed manner is familiar with the fact that each part of his body over which the water is squeezed from the sponge awakens a premonitory tingling consciousness in that portion of skin which is habitually next to be deluged."[1] Surely only the momentum of historical usage and the absence of the new psychology of thought could have prevented James from correlating his discussions of Habit, Association and Will with reference to this point!

I would once more call attention to the semi-behavioristic background of the present discussion. Studies of the delayed reaction in young children and in raccoons (possibly also in dogs[2]) have indicated the functional presence of internal factors which can initiate responses in the absence of the accustomed external stimulus. These internal factors may theoretically be either centrally or peripherally aroused processes which are called up by the sensory stimulation due to operating the release box of the apparatus. Elsewhere[3] I have given reasons for deciding in favor of the peripheral processes. The importance of the appearance in the animal series of this ability to control adaptive muscular response by internal stimuli which need not be constantly present can hardly be overestimated. Man's language sequences are but the development of the animal form of sensory associations—a development which is centralized more and more in the vocal apparatus.

[1] James, *op. cit.*, p. 555.

[2] Walton, A. C., 'The Influence of Distracting Stimuli during Delayed Reaction in Dogs.' *J. of Animal Behav.*, 1915, 5, 259–291.

[3] Hunter, W. S., 'Delayed Reaction in Animals and Children,' *Behav. Mon.*, 1913, 2, No. 1.

THE SCIENTIFIC PRODUCTIVITY OF AMERICAN PROFESSIONAL PSYCHOLOGISTS

BY SHEPHERD IVORY FRANZ

Within the past few years there have appeared reviews of the progress of psychology for different periods of time.[1] That general progress has been made is evident. That in America progress has been made is shown by the inauguration and rapid increase in the membership of the American Psychological Association, by the foundation of journals devoted to the publication of psychological discussions and researches, by the appearance in numbers of psychological books and articles, by the great popular interest which has been aroused, by the birth and growth of numerous laboratories for teaching and investigation, by the creation of special university chairs apart from those for allied disciplines (especially philosophy and education), and by the establishment of special research laboratories for applied psychology. It is also pertinent to remark that during this developing period of psychology there have been begun and continued several lines of investigation which, although not entirely American, have continued in mass and in importance to be due to workers in this country.

Although there is abundant evidence of advance, those historians who have recounted the progress have dealt with their theme in an impersonal way. They have reported the number and the character of the published investigations, the establishment of independent departments, and the number of conferred doctorates. They have not dealt with an equally important subject which is germane to the one regarding which they have written. We have not been informed by whom the psychological advances have been made, or whether or not in view of the increasing number of

[1] See C. A. Ruckmich, 'The Last Decade of Psychology in Review,' PSYCHOL. BULL., 1916, 13, 109–120. This contains references to previous reviews of like character.

professional psychologists there has been a corresponding increase in the number or in the value of the published investigations. In other words, although it is admitted that advance has been made, we are as far from knowing whether or not the advance has been satisfactory and corresponds with the number of psychologists.

The estimation of the value of an individual's contributions has been attempted from time to time and in a variety of ways. There is the well-known attempt to grade psychologists by votes of a few selected individuals. The general results of this grading have been reported.[1] There has also been a negative grading of psychologists in general in the elections to the National Academy of Sciences. Of the five so-called psychologists who have been members, three at the time of their election were professors of philosophy and primarily interested and concerned in the teaching and publication of philosophical (ontological, epistemological, and logical) doctrines. There has also been a continuing selection (and promotion) of men for professorships and other academic positions, which selection acts as a grading of a less obvious kind. The last kind of selection brings about two adverse conditions, one of them being that an individual selected for a position because of time-serving or personal recommendation has automatically acquired a claim to a grade to which his previous position and work did not apparently entitle him, the other being that because of this there has been a considerable amount of time-serving and inbreeding in some of our institutions.[2]

All of these methods of estimating the relative ranks of individuals have obvious and with respect to even their temporary value perhaps vital defects. Much the same may be

[1] 'American Men of Science.' Ed. by J. McK. Cattell. 2d ed. 1910. See especially pp. 537–596. The results have been given in only a general way; the names of the judges are not mentioned, and we must depend upon the editor's word that they were competent.

[2] While it may not apply to psychologists it is not unknown in scientific circles that some individuals, because of personal charm or characteristics such as self-appreciation and a hypercritical attitude toward others, have been able to attract students and have been able to persuade others of their importance and to get a self-enhanced reputation spread.

said regarding methods now in use for the determination of the value of a published research or other contribution. If the different methods were used in combination and subjected to safeguards respecting individual interests they would probably give better estimates than have hitherto been made. To take individuals at a given time and settle upon values or grades may be necessary, but it should be recognized that the grades shift at different times. The same holds true with regard to estimated grades or values of performed work. The supposed values of the nineteenth century do not hold for the twentieth, nor do those of today hold for tomorrow. It is, however, possible to determine whether or not a given individual or a group has carried out some of its functions, and to determine whether or not certain individuals have fallen short or have done more than their normal or expected share. We may assume as proven that in psychology there has been satisfactory advance in general, both in character and in quantity of the work, and we may inquire: "By whom have the advances been made, and in what manner have individuals or groups contributed to the advance?"

It has already been said that if we wish to deal with absolute value it is not possible to make satisfactory judgments, and answers to the questions could not be given. No one psychologist has sufficient knowledge of methods and results in all branches of psychology to be considered a sufficiently expert judge. Nor has any one the confidence of all or of a majority of psychologists. Each judge of values is influenced in making his judgments by considerations of training, of knowledge, and perhaps of special personal interest. There is no absolute impartiality. The introspectionist does not hesitate to say that behavior studies are not truly psychological. The behaviorist may reply that there is no such thing as the introspection that is talked about so much. He who does not hesitate to teach and write about the functions of the parts of the nervous system may never have performed or seen any of the classical cerebral experiments; by some psychologists the realm of the abnormal has never been visited and by them it is known only like uncharted parts in a geog-

raphy; and the remainder of applied psychology is for many like a prostitute whose acquaintance is not desired and who is to be shunned because of fear of infection.

Notwithstanding the limitations of value of individual opinion, much might be gained by taking a consensus of opinion of those of divergent special interests and of those who are admitted to be fair jurymen.[1] But we can also do something very definite by determining that a certain individual has or has not made any published contribution towards psychological advance.[2] This is a comparatively easy method giving positive results. It admits of little or no discussion of a judge's partiality, it rests solely upon the admission of published material as the facts with which to deal. We may also determine that one has contributed by books, or by the publications of investigations, or by any other means which we may select. And there is also the possibility of answering the question, "Has the progress, as measured by the number of publications, corresponded with the number of individuals who have become professional psychologists?"[3]

[1] Not related academically to the individuals to be judged, supplied with all the data respecting the individuals (not with only a part as was done in Cattell's 'American Men of Science' classification), and of sufficiently diverse individual interests to form a jury representative of all branches of psychology.

[2] There is little possibility of determining the value to psychology of the individual's college or university teaching. Some indication might be obtained by the determination of the academic antecedents of psychologists and of those who have taken advanced courses in psychology but who have not become professional psychologists. This does not mean the determination of the number of conferred doctorates. It is often forgotten that the interest of the individual has been created before he has known of the possibility of advanced work, and the doctorates give in the main only a clue to the institutions which are recommended by those who have created the interest, or which are selected because of material advantages. Another method of determining the value of the individual teacher would be that of estimating the average value and the amount of published work which his students put forth after leaving him. Not all of their subsequent work is due to his influence but an estimate could be made.

[3] The consideration of these matters has been somewhat forced upon me in connection with editorial duties during the past few years, since it was necessary to know what lines of work were being investigated and by whom. During a longer period of time I have not infrequently been asked to offer suggestions regarding possible candidates for positions, and having had very few students fitted for such positions, I have taken the opportunity to recommend those who have exhibited some accomplishment (publication) rather than those who were known to me as individuals of 'promise' in their advanced work. The results of some of these casual inquiries regarding accomplishment were so different from what I had expected that it led to the consideration of a larger group.

We have available for the purpose well-known yearly bibliographies, and the present article has been made possible because of them. The membership list of the American Psychological Association was consulted to obtain a list of our 'professional' psychologists. Since membership in the Association is not limited to those who are instructors or professors of *psychology*, only those whose official positions consisted *solely* in relation to psychology were considered to be the professional psychologists.[1] Since those few who hold only research positions can not be dealt with in the same manner as teachers, I decided to omit their names from the list to be investigated. This left 87 names, of which three were omitted because of the lack of certain data which I thought essential. The 84 individuals represent 48 institutions, 45 as professors, 26 as assistant, associate or adjunct professors, and 13 as instructors. The academic titles mean little but they are mentioned only to indicate the range or the number of departments of psychology. Five individuals were connected with one institution, four with another, there were three in each of nine institutions, there were two in each of eleven, and one in each of twenty-six institutions. Half of the men had received their doctorate during the decade as follows: 1906, 2; 1908, 6; 1909, 6; 1910, 4; 1911, 2; 1912, 10; 1913, 7; 1914, 4; 1915, 1.

Since my immediate interests have been connected with recent publications I selected the past ten years, 1906 to 1915, inclusive, and have tabulated the contributions of each of the 84 individuals for each year. The contributions which have been listed were found in the PSYCHOLOGICAL INDEX.[2]

[1] The 1916 membership list was used. This contains the titles of positions as supplied by the members, and is presumably correct. I am convinced that some of the titles are incorrect, but the mistakes are relatively unimportant. I have not gone beyond the official returns. Some professional psychologists who are not members of the Association are not included. Some of them are known to the writer, but it seemed unfair to include them, since they are not members of the national body of professional psychologists.

[2] This bibliography does not give references to all of the publications of psychologists, some contained in inaccessible periodicals are not given, some which are not psychological are not noted, but the failure to list all the psychological publications of any one individual rests solely with that individual, since the INDEX asks that omissions

The different kinds of contributions have been listed under the following titles: Monographs, Original Articles, Discussions, Books, General Reviews, Reports of Meetings. A few translations of American books into foreign languages were omitted, on the ground that the original authors had already made the contribution and the work of translation was that of another. Translations of foreign books into English were also omitted from consideration. In some cases it is difficult to decide whether a certain publication is a discussion or an original article, in other cases whether a publication be an original article or a monograph, or a monograph or a book. Some periodicals publish long articles which in other series would be published as monographs, but they have been dealt with as they stood. When doubt arose as to the character of the publication the contributor was given the benefit of the doubt.[1] The inclusion of general reviews and reports of meetings may need justification. They have appeared to me to be legitimate methods of advancing psychology because they may create interest to investigate certain matters or give facts to others which would not ordinarily be discovered, etc. Their value, however, must be considerably less than those of publications containing the results of personal investigations. Somewhat similarly with books, since they are largely compilations of the work of others.

Table I. gives the results of the examination of the INDEX for the individuals and the decade under consideration. Here there are shown the numbers of each kind of publication for each year and for the ten-year period. It will be seen that with the exception of a few lean years (1907, 1914) the number

and corrections be supplied. A few mistakes were noted, when they were obvious they were taken account of in the tabulations. The tabulations by years are not always exact on account of the inability of the INDEX compilers to secure all the references for a particular year at the time of publication, but titles omitted in one year are found in the next number.

[1] That is, as will be noted below, an artificial value was assigned to each kind of publication, and when doubt arose the higher value was assigned. At the same time a 'joint' article, etc., was assigned to both individuals, and given in each case its full value. I was at first inclined to reduce the article value for 'minor contributions,' but I concluded to let the above consideration hold. The recent publication of the Proc. Nat. Acad. of Sciences was another difficult matter. The 'articles' in that journal are in reality abstracts or summaries (*Auto-referate*).

TABLE I

KINDS OF PUBLISHED CONTRIBUTIONS OF PROFESSIONAL PSYCHOLOGISTS FOR THE DECADE, 1906–1915, INCLUSIVE

	1906	1907	1908	1909	1910	1911	1912	1913	1914	1915	Totals
Monographs	1	3	2	2	5	5	5	10	6	6	45
Articles	46	36	43	49	42	44	59	58	39	76	492
Discussions	7	4	5	7	2	8	9	12	9	3	66
Books	2	1	10	6	3	6	8	5	9	5	55
Reviews	4	1	4	3	4	29	32	25	26	33	161
Reports	1	0	4	3	4	6	3	2	4	5	32
Totals	61	45	68	70	60	98	116	112	93	128	851

of research articles and monographs has not differed very greatly from year to year and there is a fairly gradual increase. There is a slight increase in 1912 and 1913 over the preceding years and a greater increase in 1915 over 1913 and 1912. The notable decrease in 1914 is not explained.[1] It may be that the increase of interest in the subject of tests is responsible, those who had taken up this line of work being occupied in 1913 and 1914 with the accumulation of facts which were published in 1915.[2] When we look at the yearly totals we note that the increase began in 1911, and this was coincident with the inauguration of the series of general reviews in the PSYCHOLOGICAL BULLETIN.

Table II. shows the distribution of the contributors over the ten-year period. It is a remarkable fact that the first five years are almost constant in number of contributors and that the increase in the number of contributors has taken

[1] A writer in the *New York Times* book review supplement has mentioned that in general literature 1914 was a lean year. The reason is not obvious in that case or in the case of our psychological publications, since the European war could not have its effect on production until very late in the year. In connection with psychological journals this effect should have been more noticeable in 1915 if it existed because of the war.

[2] Ruckmich reports over 800 *original* articles for the decade 1905–1914. Probably the number in 1905 was nearer that for 1906 than that for 1915 of the present table. On that assumption we would have about 500 titles of *original* articles and monographs for that decade by professional psychologists. Not all of these have appeared in the magazines listed by Ruckmich, and in comparison with the 'over 800' noted by Ruckmich it is apparent that fully 3/8 of the grand total was contributed by the non-professional psychologists, or by those whose loyalty was divided between psychology and philosophy, education, etc.

TABLE II

GROUPING OF PROFESSIONAL PSYCHOLOGISTS ACCORDING TO YEARLY NUMBER OF PUBLICATIONS, FOR THE DECADE, 1906–1915, INCLUSIVE

	1906	1907	1908	1909	1910	1911	1912	1913	1914	1915
1 contribution	15	16	12	11	17	14	20	17	19	28
2 contributions	10	7	8	8	8	9	13	13	13	12
3 contributions	3	3	5	6	3	7	5	5	8	6
4 contributions	0	0	1	2	2	0	2	5	2	6
5 contributions	2	0	1	1	2	6	1	3	2	1
6 contributions	0	1	0	2	0	0	2	2	1	2
7 or more contributions	1	0	2	0	0	2	4	1	0	2
Totals	31	27	29	30	32	38	47	46	45	57

place during the past five years. The greatest number of contributors is found in 1915, the year of the greatest number of contributions. This table also shows what may be termed the scientific activities of psychologists for it gives the number of publications of groups of individuals. For the past five years about 30 per cent. of those who contributed published three or more articles, etc., each year. This is, of course, not to be taken to mean that the same individual did this from year to year, although it may be mentioned that the tabulation of the material indicates that the man who does it one year is more apt to repeat with more than a single contribution for the following years.

Since not all of the individuals on our list have been, nor could they be expected to be, active in publication during the whole of the decade it is of interest to compare the figures which are given in Table II. with the examination of the years of the doctorate or other higher degree which is held. There were 42 individuals who had obtained their higher degree anterior to 1906.[1] The other 42 could not be expected to publish work before their doctorate, although some did so. Counting the date of the doctorate as the date when publication might reasonably be expected, the numbers of individuals expected to publish were obtained by adding the new doctors to the original 42 at the beginning of the decade. It has already been mentioned that some published before the doctorate, the number being 14 (out of a total of 42), and a few published more than one article.

[1] Two individuals on the list hold the Master's degree only.

When now we compare the number of expected contributors with the actual number of contributors we find an interesting condition. Table III. gives the data. Although the number of expected contributors increased during the first five years the yearly number of actual contributors did not increase. The increase in contributors came during the second half of the decade, but the percentage of expected contributors who published is the same (68 per cent.) at the beginning and at the end, there being a decrease in the percentage up to the year 1910 and an increase thereafter. If we separate out those contributions which are intended to convey new facts or new interpretations,[1] which would limit us mainly to articles and monographs, we find an even more

TABLE III

THE PUBLICATION ACTIVITIES OF PROFESSIONAL PSYCHOLOGISTS FOR THE DECADE, 1906–1915, INCLUSIVE: THE NUMBERS OF EXPECTED CONTRIBUTORS, OF ACTUAL CONTRIBUTORS, OF THOSE CONTRIBUTING ARTICLES AND MONOGRAPHS, AND THOSE CONTRIBUTING PREVIOUS TO DOCTORATE

	1906	1907	1908	1909	1910	1911	1912	1913	1914	1915
Expected contributors	44	44	50	56	60	62	72	79	83	84
Actual contributors	30	26	26	28	29	37	43	45	45	57
Contributors of articles and monographs	28	24	23	26	28	27	30	34	35	40
Contributors previous to doctorate (additional)	1	1	3	2	3	1	4	1	0	0

interesting comparison. At the beginning of the decade there was a total of 44 individuals expected to contribute. Of this number 28 made contributions of articles and monographs (64 per cent.). The percentage (actual contributors in relation to expected contributors) decreased in the following years as follows: 55, 46, 46, 47, 44, 42, 43, 42, 48. The contributions anterior to the doctorate by those who contributed previous to their doctorate are not included in these calculations. It is to be observed, therefore, that the percentage of original contributors has decreased and the percentage of total contributors at first decreased and later reached its original figure. The great differences in the percentages from 1911 to 1915 inclusive are to be understood

[1] It is not intended to say that books, discussions, reports or reviews do not contain new facts and new explanations, but that they are less apt to do so.

primarily as the result of the publication of general reviews and an apparent satisfaction of the authors of these reviews in their accomplishment.

The subject of books deserves a separate paragraph. These were mainly the work of the group of older men, for of the 55 books which have been published in the decade only five have been written by those whose doctorates were granted in 1906 and subsequently. Two men were responsible for one book each, two for two books each, four for three books each, one for four books, and one for fifteen books. The years of publication of the 55 books are shown in Table I.

Mention has already been made of the increase in the expected contributors owing to the granting of the doctorate. The original 42 names have had added to them an equal number. Of these additional 42, 14 published previous to the doctorate. The first publication after the doctorate, in most cases dissertations or parts of dissertations, of 18 was made in the year of the doctorate, of 10 the first publication was in the year following the doctorate, of 9 in the second year, of 1 in the third year, of 2 in the fourth year, and of 1 in the fifth year after the granting of the doctorate. No publication by one who received the doctorate in 1913 had been made up to and including 1915. These figures show that either our means of publication are insufficient, or that the dissertations presented in partial satisfaction of the doctorate are not nearly ready for (or are not worthy of) publication, or both.[1]

The 42 individuals who had received the doctorate prior to 1906[2] might each have contributed something in each of the ten years under consideration, but only seven did so. Seven others contributed in 9 of the ten years; 4 in 8; 4 in 7; 6 in 6; 1 in 5; 2 in 4; 3 in 3; 2 in 2; 2 in only one of the years; and 4 had no publications. Those who received the doctorate in 1906 and subsequently can not be dealt with in the same man-

[1] It might be well for university authorities to deal a little more strictly with the matter of publication. The publication of a dissertation in part or as a whole is the only evidence to the world outside of the particular university that the individual has shown a capacity for investigation, one of the main doctorate requirements in all institutions of which I have knowledge.

[2] Including the two who have not taken the doctorate.

ner, but the groups in accordance with the percentage of years in which contributions were made, counting the total of years since the doctorate as the expected total, are as follows: 0, 1; from 1 to 10 per cent., 7; 11 to 20 per cent, 5; 21 to 30, 4; 31 to 40, 5; 41 to 50, 13; 51 to 60, 1; 61 to 70, 1; 71 to 80, 3; 100 per cent., 8. The number of older men who averaged at least one contribution for every two years, or more often, is double that of the younger men. The younger men had more than two thirds of their number who did not publish as much as one contribution for every two years. It should further be stated that of the younger men placed in the most regular class (100 per cent.) one contributed original work in only one of 8 years, a second in only two of 6 years, a third in six of 7 years, and a fourth in two of 3 years. The other 4 contributed an article or monograph in each of the expected years. Of the older men in the 100 per cent. class, one failed to report original work in only one of the ten years, one did not report such work for two of the years, and two for three of the years. Three contributed at least one article or monograph in each of the ten years.

Comparing the two groups we find that of the 420 expected individual years of publication of the older group (10 years each for 42 individuals) there were only 257 individual years of publication, a percentage of 61; of the younger group there were 214 expected years and an actual total of 109, a percentage of 51. This difference is entirely accounted for by the long delay in publication after the doctorate, for if the delay periods be subtracted from the total there is a percentage of 61 for the younger men. Although there is not a sufficient number of years to make the conclusion certain, the figures would lead to the belief that when the younger men start there is not a great difference in total productivity between them and the older men. My impression previous to tabulation was the reverse, probably because of some notable examples of productivity of the older men. The latter is counterbalanced by the fact that there are five of our professional psychologists, four of the older group and one of the younger group (the latter case mentioned above), who have

not published anything worthy of citation in the PSYCHOLOGICAL INDEX in ten years.

The individual differences which have been mentioned are better shown when we compare the totals and yearly averages for the individuals of the groups. It would not be expedient to mention names of individuals, or to designate them in recognizable terms, so that we must fall back upon generalities of individual differences. Of the older group there are four who did not make a scientific contribution of such a character or in such a journal as to be deemed worthy of mention in the INDEX in the decade; there is only one of the younger group. In addition the contributions of one of the older

TABLE IV

DISTRIBUTION OF PROFESSIONAL PSYCHOLOGISTS ACCORDING TO THE NUMBERS OF CONTRIBUTIONS IN THE DECADE, 1906–1915, INCLUSIVE

Number of Publications	Total Publications		Articles and Monographs	
	Older	Younger	Older	Younger
0	4	1	5	1
1 to 10	14	35	16	38
11 to 20	12	4	16	2
21 to 30	5	1	4	1
31 and over	7	1	1	0

TABLE V

COMPARISON OF TOTALS OF PSYCHOLOGICAL CONTRIBUTIONS BY THE OLDER AND YOUNGER GROUPS OF PROFESSIONAL PSYCHOLOGISTS FOR THE DECADE, 1906–1915, INCLUSIVE

	Articles	Monographs	Books	Discussions	General Reviews	Reports	Totals
Older	369	19	50	55	109	18	620
Younger	123	26	5	11	52	14	231

group consisted exclusively of the class of general reviews and reports. The accompanying tables show the distribution of the men in accordance with their scientific-literary productivity. In Table IV. there are shown the numbers of individuals in each group in relation to the total number of publications and in relation to the publications of articles and monographs. In Table V. there are shown the different

kinds of publications (articles, monographs, books, etc.) according to the groups. The average total number of contributions for the decade by the older men is 14.8, for the younger men it is 5.5; the average of articles and monographs by the older men is 9.2, and by the younger men only 3.5. These figures may be somewhat misleading if taken as they stand, on account of the difference in the total number of years that might be expected for scientific publication. The total years for the older group has already been said to be 420 (42 individuals for 10 years), and 214 for the younger group. When the comparison is made of total publications and of original (monograph and article) publications of the older group and of the younger group in relation to the expected number of years it is also found that the older group outranks the younger. Thus the individual yearly average for total contributions for the older group is 1.5, and only 1.1 for the younger group; and the individual yearly averages for articles and monographs are respectively .92 and .69 for the older and the younger men.

It is interesting to speculate on the reasons for these differences. Doubtless in most institutions the younger men are employed a greater part of the time in preparation of materials for the laboratory work of students and in the grading of themes, etc. In the smaller and less well endowed institutions there is less aid for the prosecution of investigations, and if aid can be obtained it takes an exorbitant amount of time to get the administrative machinery in running order. At the same time the younger group has to take considerable time in the preparation of material for their courses of instruction, and they are more frequently called upon to act as subjects or assistants for other research workers. On the whole the older group has the advantage of long established policies, of equipment, and of professional and mechanical assistance. At the same time the labor of teaching is correspondingly less on account of the previous experiences. Perhaps if we should compare the work of the older group, or of as many of them as held positions in the decade of 1891 to 1900, with that of the present younger group we should not find as great differences as now exist.

On account of the differences in time opportunity it is to be expected that the younger group would publish less in total and that the individual total would be less than many of those of the older group. Thus we find that nearly one half (19) of the older group reached or exceeded the average of 1.5 publications per year, and that only one third (12) of the younger group equalled or exceeded the yearly average for that group (.5 publications per year). Table IV. gives the results of the groupings. This shows that more than one half of the older men averaged one publication per year, seven having more than three per year The figures for the younger group must be read in the light of the total yearly expectation (214 instead of 420). Each individual should be considered in relation to the total of his expected years of publication. Thus the six individuals of the younger group who exceeded ten publications had a total publication expectation of 44 years. The total of publications was 124, which gives a yearly average less than 3.

The data with regard to the character of the publications of the two groups are given in Table V. The difference in the number of books has already been mentioned. Monographs are apparently the prerogative of the younger man, the newly created doctor, for he has in proportion to the expected years two and one half times as many as his older colleague. The older men publish 33 per cent. more articles, as has been mentioned, they take part in more controversies as judged by the number of discussions, and they contribute an equal share of the general reviews. With regard to the last the remark may be necessary that the general review is most frequently due to 'request' and not infrequently a request to the elder is declined in favor of a younger colleague.

Since the different kinds of contributions are so varied no direct comparison may be made of individuals except in terms such as have already been used (total number of contributions, number of monographs and articles, and the relation of these to the expected years of publication). An indirect comparison may, however, be made if we assign to the different classes of publications an arbitrary numerical value. This

must be very arbitrary on account of the impossibility of making good comparisons of values, as has already been explained. It is also to be kept in mind that no arbitrary value for a class can be defended in particular cases, for if we consider two articles we may immediately note that one deals with an investigation in which some new methods have been used and the analysis of factors appears to be well wrought out, and we may also find that the second consists of a confirmation of previous work by the use of the same methods which had previously been used. Even in two articles which contain new facts or demonstrate new methods we may find similarities and divergencies of completeness or of apparent originality. It favors the majority of low grade (if we may use such a term) publications if we assign the same arbitrary value to all of one class, and this I have done. The values which I have selected for the different classes of publications are as follows: Reports of Meetings, 1; General Reviews, 2; Discussions, 3; Books, 6; Articles, 6; Monographs, 9. No justification of these arbitrary values will be attempted, but the following were in mind when the values were assigned. A discussion is often of no observable value in adding to our knowledge, and scientifically is worse than useless when it takes on the character of a personal attack. On the other hand, it may tend to clear up doubtful points, bring up new ways of viewing a situation, and at the same time by pointing out gaps in our knowledge indicate lines of investigation. In so far as a discussion does any of the latter things it appeared that it has a real value beyond that of a report of a meeting, and since a discussion also tends at times to inter in a suitable manner some supposed facts which very generally, but erroneously, have been accepted it was thought worthy of a greater value than that of a general review. It is assumed that all of our psychological discussions are of the good character mentioned. Differences exist in general reviews. Some are summaries of a few contributions of others. Some give a fairly complete account of current work with an evaluation of the material and thus help others who are not specializing in the subject to obtain a better view than would be ob-

tained by looking over the mass of details which the original sources contain. Some of the general reviews which we are considering here may be worthy of a higher value than some of the discussions, but many might bear a reduction of the general value figure. Here again it became necessary to decide for the majority rather than for the few. Much the same may be said with regard to books. Although the writer does not pretend to have the specialist's critical ability in every branch of psychology he is satisfied that many books published during the period are nothing more than general reviews, and at times poor ones. Some of the books have brought out new facts and explanations, they have added considerably to our psychological advance. But because of their general character it is thought that a value double that of a discussion and triple that of a general review would be an ample average value. The original, or research article (whether experimental or otherwise) was assigned a value equal to that of a book, and the monograph (but only on account of its length and supposed completeness) was assigned a value fifty per cent. higher. The remark previously made regarding monographs, that some monographs would be articles in other publication series, is a point against such a valuation, and the valuation is not insisted upon. The increase in value is on the side of the younger men, who most need the extra count.

Having selected the arbitrary values which have been mentioned it becomes possible to make comparisons of the output of different individuals without making special reference of an identifying character. At the same time it becomes possible to make comparisons of the groups since the heterogeneity of the different kinds of publications has been translated into a homogeneity.

The calculation of the individual values for the ten-year period shows that the range is from zero to 244. This means that some have contributed nothing and that others range from the zero point up to a valuation of 24.4 points per year. It is necessary to translate all the total valuations of the younger group into 'expected' values, or average yearly

values in accordance with the number of expected years of publication. When this is done for all individuals we find that there is a general average of 6.7 points per year for the group as a whole, and a median of 4.2. When the five individuals who have not contributed even as much as a report of a meeting during the decade are omitted the average is 7.2 and the median is 4.5. The distribution of the 84 individuals in respect to average yearly values of publications is given in Table VI. Eighteen of the older group and twenty-five of the younger group are below the median of all values; 22 of the older and 30 of the younger groups are below the average of the total. Arranging all in sequence of average yearly values and dividing into four equal parts, each containing 21 men, we find the following distribution of the older and younger respectively in the groups from lowest to highest: older, 10, 7, 11, 14; younger, 11, 14, 10, 7.

TABLE VI

GROUPING OF PROFESSIONAL PSYCHOLOGISTS ACCORDING TO AVERAGE YEARLY "VALUES" OF CONTRIBUTIONS FOR THE DECADE, 1906–1915, INCLUSIVE

Values	1.0 and Under	1.1–2.0	2.1–3.0	3.1–4.0	4.1–5.0	5.1–6.0	6.1–7.0	7.1–8.0	8.1–9.0	9.1–10.0	10.1–15.0	15.1–25.0
Number of men.....	9	14	9	9	9	2	0	5	5	4	9	9

The older group on account of time, material equipment, academic relations, and other conditions has advantages which make it of special interest. All of the individuals had attained their higher degrees previous to 1906, about three quarters are heads of departments, and about the same number are connected with well-equipped and long-established laboratories. This group furnished four individuals who did not make a published contribution of any kind to psychological advancement in the decade. Six of the group contributed one original article or monograph during the ten-year period; two contributed two original contributions; two contributed three; and three contributed four. We have in this group, therefore, forty per cent. who have not averaged an original contribution once in two years. Some of these seventeen

individuals did contribute in other directions besides monographs and articles, for they published 5 books, 33 general reviews, 2 discussions, and wrote one report of a meeting. Besides the four who did not make any kind of a contribution there were four additional who made no contribution beyond the original articles and monographs published. On the whole the younger group, while not as productive as the older, show better results with respect to the publication of individuals. Only one (duration three years) has not published, as is mentioned above. Ten others have not published as frequently as once in three years; there is a total of sixteen who have not published as frequently as once in two years.

Since the older group had the opportunity to publish for ten years we need not deal with averages entirely, but may consider totals as well, on account of the homogeneity of the series. It is of interest to know that seven of the group (17 per cent.) contributed a total of 159 articles and monographs (41 per cent.); the highest half of the group (21) contributed eighty-seven per cent. of all the articles and monographs of the group (337 articles and monographs). This leaves for fifty per cent. of the older men only thirteen per cent. of the articles and monographs published by the group.

The majority of older men who have contributed little in the way of articles and monographs, and also to the total, have held their present positions for many years, and have apparently 'grown up' in their present locations, they are located in some of our better endowed institutions, and in those with good laboratory facilities, they have colleagues teaching in the same lines, and they occupy what may be properly called positions of prominence in their respective institutions. They have not the apparent disadvantages of isolation, or of having to carry the burden of the psychological world upon their shoulders since there are colleagues to help in teaching and perhaps in research. But these are some of the men who are representing psychology as a science in their respective university niches. Those of the younger group who are least productive, and have given little evidence

of interest in psychological advance by publication, are mostly located in the smaller institutions, where there are no colleagues of sufficient training or productiveness to be elected members of the American Psychological Association. There are in the younger group exceptions to this, several notable cases being evident when the list is inspected. Without going into the figures for total or original publications, since the number of men at the different institutions is small, it may be stated that those younger men who have the opportunity to remain at a long-established department have done better than those in the more recently created departments. But this does not hold for particular cases, since there are surprising exceptions of individuals with apparently all the advantages which can be obtained in the better endowed institutions doing little or nothing which is prepared for the edification of their scientific colleagues.

It should not be assumed, and it is here stated to the contrary in order that there may be no misunderstanding, that these men are doing nothing for psychological advance. Some may have editorial duties, some may conceal themselves in the work of their students, and some (like Herbert Spencer) may be reserving their energies for some *magna opera* which will be given to the world in due time. It seems unlikely, however, that as many as 40 per cent. of the older group are engaged in the accumulation of material for the development of a cosmology, or of a system of psychology, or of an exhaustive history of the science, or of other large projects which should not be laid aside in favor of the minor contributions such as articles and monographs. It is apparent that a few of those who hold chairs of psychology are contenting themselves with teaching and the carrying out of the social obligations which fall to the lot of every scientific man, who must meet his colleagues and take part in the life of the university with which he is connected. Whether or not there are more psychologists who are doing these things than scientific men in other lines can not now be determined. And, the other view may be expressed that those who are contributing much do so without proper scientific care and because of reportorial

tendencies. The character of the work of many of our most productive men shows that they have not done their work at the expense of care. But, the writer feels that some of the so-called 'professional' psychologists should be classed with dilettantes; they are not scientific professionals in the sense that they are forging ahead and that they are succeeding because of their efforts in scientific work.

Much has been written recently about university positions and university control; the professor always being the oppressed and the university as represented by the president being the oppressor. I have a feeling that part of the dissatisfaction may be due to the 'great promise' for which candidates have been recommended never showing up in practice. A president of one of our leading institutions has been criticized for saying that every man on the faculty may be expected to publish at least one article every two years. It may be that he had had experience with those of 'promise' and not of performance. Much has also been written about academic tenure, as if that was a sacred right (or rite), but the critic might well imagine a better state of affairs to follow in some of our universities if there was a power of recall for those members of the faculty who do not measure up to the expected or to the average amount of performance. If the present methods of filling positions, from the grade of professor down to that of instructor, be continued we shall always have some of little or no accomplishment (the dilettantes) filling important chairs, especially if there be the added conditions that promotions be made as vacancies occur. It is the writer's belief that much of the inconsistencies would be eliminated if every vacancy were advertised as open to application, stating that applicants were expected to send in the accounts of their academic careers, copies of publications, etc., and that these would be graded by a non-interested committee of specialists (advisory board) who would report to the faculty or to trustees the results of their findings. We should probably escape the absurdity of having in one year two young men recommended from one university as 'geniuses of the first water.' This has happened, and the writer is not certain which is to be the more pitied, the university which permitted its 'geniuses' to go to other institutions, or the universities which would accept men who were recommended as such.

Another matter may be worth considering briefly. This is the relation of productivity to the institutions from which the higher degrees were received. In pointing to psychological advances in institutions, we have hitherto been content to mention the number of doctorates which have been granted. But it is obvious that numbers count for very little in progress, unless one is going to play a mass game or overrun a weak nation. What should be considered is the question: What have the doctors done after leaving an institution? And we may also ask: Has the training which is supposed to fit the man for research been effective in stimulating him to efforts in that direction? Part of these ques-

tions may be answered by considering the performance of those granted higher degrees by different institutions. Of the 84 men on the present list 17 were scattering with respect to institutions, but the remainder were distributed over seven institutions. Only one institution is mentioned by name (Leipzig), the other individual institutions (American) are represented by letters. The 17 scattering cases are grouped together as 'other foreign' and 'other American.' Table VII.

TABLE VII

COMPARISON OF PUBLICATIONS OF ALL KINDS AND OF ARTICLES AND MONOGRAPHS, BY GROUPS OF PROFESSIONAL PSYCHOLOGISTS, ARRANGED ACCORDING TO INSTITUTIONS CONFERRING DOCTORATES. (TWO INDIVIDUALS WHO HAVE NOT RECEIVED THE DOCTORATE ARE INCLUDED UNDER THE INSTITUTIONS WHICH CONFERRED THEIR HIGHER DEGREES)

Institutions	Leipzig	*A*	*B*	*C*	*D*	*E*	*F*	Other Foreign	Other American
Numbers of men	8	11	7	15	11	8	7	6	11
Total expected years	78	79	54	132	74	58	42	57	60
All publications:									
Total credits	859	674	421	884	381	226	103	422	269
Credits ÷ years	11.0	8.8	7.8	6.7	5.1	3.9	2.5	7.4	4.5
Range of average yearly credits of individuals	3.6–18.6	1.5–19.3	1.1–23.0	0.0–24.4	1.4–14.0	0.6–11.3	0.0–7.5	0.6–9.4	0.0–10.2
Articles and monographs:									
Credits	603	510	360	708	312	198	96	348	222
Credits ÷ years	7.7	6.5	6.7	5.4	4.2	3.4	2.3	6.1	3.7

gives the numbers of men from different institutions, the total years under consideration (the total 'expected' years of all in the special group), the total credits, the relation of credits to the total expected years of publication, and the minimum and maximum average yearly credits by the individuals in the groups.

Probably the total expected years, rather than number of individuals, is a better means of comparison with the totals of publications, and the relation of these two are shown in the fifth line. Here it will be observed that the 8 men from Leipzig far surpass the groups from the other universities. They have the highest minimum, showing that each is publishing fairly regularly, and although they are surpassed in maxima by three other institutions this is largely due to a few very

productive men. The men from Leipzig have mostly been of the older group, as indicated by the average of expected years (9.8), but the average is not much greater than that (9.5 years) for the group from Other Foreign Institutions. Leipzig also closely approaches the total of *C*, although the number of men is only slightly over half, and the total number of expected years is about 60 per cent. of this American university. The order of institutions in total credits divided by expected years is as follows: Leipzig, *A*, *B*, Other Foreign, *C*, *D*, Other American, *E*, and *F*. When now we compare the performance of the doctors from the different institutions in relation to publication of articles and monographs we find the results shown in the last two lines of Table VII. Here again Leipzig shows a great superiority. The order of institutions in credits divided by years has not materially changed, there being a reversal of the order for *B* and *A*. The remainder of the table speaks for itself.

One matter remains to be briefly considered. This is the relation of administrative work to the carrying on of scientific production. Our group contains five individuals who have given their occupation as partly that of dean or president. Two of these individuals notwithstanding their arduous administrative duties have managed to carry on investigations and to publish the results of them. At the same time they have both for total and for original publications managed to be in the more productive class. The other three probably have their administrative duties as reasons for their relative non-performance. In the group there are also eleven who have greater or less editorial duties in connection with the publication of periodicals. Of this group two have been below the average in performance. Both of these extra-scientific duties (administration and editing) have not interfered with the production of an average amount of scientific articles by two thirds of the men thus engaged, and it may be that these added functions have been beneficial rather than the reverse. In the estimation of productivity of individuals editorial functions have been omitted and had these been added to the totals in some numerical way it is quite likely

that more of the group of editors would have approached the top. As the arrangement now stands, counting only articles, books, reviews, etc., we find that two thirds are in the higher half of the distribution, and of these most are in the higher quarter.

It is pertinent to remark that psychology appears to be getting from those whose chief interests are not in its development (from the non-professional psychologists, in other words) as much as, if not more than, from many of its own men.[1] From some of its own psychology is receiving much less than should be expected, and there is, perhaps, some reason for the assertion that "were it not for an academic title it would be difficult to discover the reason why certain individuals are called psychologists." In conclusion the attention of the reader is called to the consideration of the wisdom of the action of certain scientific societies which require that a member shall retain membership in them only as long as he continues to show an active interest in the advancement of his science by publication, provided continued ill-health or other disabling conditions (old age and the like) do not prevent.[2]

[1] I expect to deal with the psychological contributions of these non-professional members of the Association in another article.

[2] A constitutional amendment of this character was recommended by its council to the American Psychological Association at one of its meetings, and at the subsequent meeting the council reversed its recommendation (see the Proceedings of the Association for the years 1906 and 1907).

THE PSYCHOLOGY OF THINKING IN THE CASE OF READING

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If a person is presented with a paragraph to read and questions about it, his responses provide useful material for studying some of the facts and laws of thinking. Consider, for example, the following task and the following responses made to the first question by pupils of grades 5 and 6:

1

Read this and then write the answers to 1, 2, 3, 4, and 5. Read it again as often as you need to.

Nearly fifteen thousand of the city's workers joined in the parade on September seventh, and passed before two hundred thousand cheering spectators. There were workers of both sexes in the parade, though the men far out-numbered the women.

1. What is said about the number of persons who marched in the parade?
.....
2. Which sex was in the majority?
.....
3. What did the people who looked at the parade do when it passed by?
.....
4. How many people saw the parade?
.....
5. On what date did the event described in the paragraph occur?
.....

Two hundred people
Three thousand
Thousand
Eighteen thousand
Two thousand
Five thousand
Ninety thousand
Twenty-five thousand
About thirty-five thousand
Nearly twenty thousand
More than ten thousand
There were about 25000
200,000
It was 200,000
About two thousand
Maybe No. 12
About 2700
Two hundred thousand spectators workers in the parade
Two hundred thousand spectators
Two hundred cheering
Nearly 115000 on Sept. of people
Nearly sixteen thousand
Hundred thousand spectators
It is said about the number or group of people
It is said that they are great
A very great deal

A lot of people
Congregation
There were a great lot of men
The men outnumbered the women
The men were more than the women
There were more men
They outnumbered the women
There the par on number the
The men were far ahead of the women
Men and woman
Citizens
They were workers
There were workers of both sexes
Workmen in the parade
Of all the working men
That the city workers joined the parade
Workers joined the parade
That they rejoin in the parade
They were joined
A number of workers joined the parade
Joined the parade
Workers join
They joined
They pass two hundred spectators
Before the spectators
Passed before two hundred thousand spectators
They two hundred thousand cheering spectators
Passed before 200000 and 15000
They passed nearly 5000
Passed before two hundred spectators
They marched before cheering spectators
Three thousand cheering them
People of both sexes cheering them
They are cheered
Parade before two hundred spectators
Parade spectators
They marched nice
They marched very nice
They kept in step
They marched very straight
They did good or bad
They look so nice
They clap their hands when they see the American flag
They keep their step and many others
There character
Honorable and good
The people said the parade large
Most of them were old
They are soldiers and marched
They say halt
The captain says march
There was a lot of floats
The people are killed by the war
The meddles
September seventh
Irish

The variety of responses to this one fairly unambiguous question is a challenge. There is a challenge also in the relative frequency of the different responses.

I shall report here some general facts which are displayed by some hundreds of responses to each of a dozen or more sets of questions upon a paragraph, which I have examined. The first is that:

When a question on a paragraph is answered, any one word may be over-potent in determining the response. As a limit we have the case where a word produces a response due to that word alone irrespective of all else in the situation. Or, more generally, any element in a situation may be over-potent to any degree.

The evidence supporting this claim is the existence of

answers or elements in answers which could come as probable results or over-potent action of single words, but whose occurrence otherwise is highly improbable. For example, in the case of the words of the paragraph such influence is seen of:

thousand, in this answer to 3: "The people cheered thousands."
city, in this answer to 2: "City workers."
workers, in these answers to 2: "Workers," "Workers of sex"; and in this answer to 3: "There were workers."

In what follows a number in parentheses preceding a response designates the question as an answer to which the response was given.

joined, in (1) "They joined," (1) "They were joined," (3) "They joined in," (3) "They were joined in the parade," (3) "They cheered then joined in."
parade, in (2) "in the parade," (2) "Both in parade," (2) "Sexes in the parade."
September, in (2) "There were workers of the September."
seventh, in (2) "Seventh," (3) "September seventh," (3) "Seventh Avenue."

Lest the bizarre nature of some of these errors lead the reader to fancy that they are fragments, or answers misplaced a line too high or too low, I may note here that every quotation that has been or will be given in this article is, unless specially noted at the time, a complete answer, as given by some pupils, and undoubtedly intended for the question whose number it bears. Quotations are exact except that the first word is capitalized whether or not this was done by the pupil.

passed, in (3) "Two hundred thousand cheering spectators passed."
spectators in (1) "Parade spectators," (1) "Cheering spectators," (1) "Two hundred thousand spectators workers in the parade," (2) "Spectators and working," (3) "The people who looked cheered the spectators," (3) "The people looked and cheered the spectators," (2) The sex spectators.
cheering, in (2) "Cheering."
both, in (2) "There were both workers," (3) "There were workers in both spectators."
sexes, in (3) "Six in the parade," (3) "Sixes," (3) "Cheered the sex in the parade."

though, in (2) "Though the men marched in the parade," (2) "Though the men far outjoined parade."
men, in (3) "They cheered the men in the parade."
far, in (2) "Far out women."
out, in (3) "They counted out how many women."
numbered, in (3) "The men numbered women," (3) "Numbered the women," (3) "They numbered the people."
out-numbered, in (2) "Sex out numbered of women," (2) "Out-numbered."
women, in (2) "Sexes of women," (2) "Men and women," (3) "The women."

Any phrase or other part of a sentence tends in a similar manner to produce a response due to that phrase per se.

Thus, *fifteen thousand* appears as an answer to questions 2, 3 and 4 and (in "Passed before 200,000 and 15,000") as an answer to question 1. The following are responses probably explainable by the independent action of phrases or other groups of words.

the city's workers, (2) "City workers."
joined the parade, (1) "Joined the parade," and (2) "They joined the parade."
September seventh, (1) "September seventh."
passed before two hundred, (3) "Passed before two hundred."
two hundred, (1) "Passed before 200 spectators," (2) "Two hundred spectators," and (4) "Two hundred."
hundred thousand, (4) "About a hundred thousand."
two thousand, (1) "Two thousand," and (4) "Two thousand."
two hundred thousand, (1) "Two hundred thousand," (2) "Two hundred thousand," and (3) "Two hundred thousand."
There were workers, (2) "There were workers."
of both sexes, (1) "People of both sexes cheering them," and (5) "Sept. 7th both sexes."
There were workers of both sexes, (1) "There were workers of both sexes," and (3) "There were workers of both sexes in the parade."
though the men, (3) "Though the men far out joined parade," and (5) "Thought the man fat out."
outnumbered the women, (1) "They outnumbered the women," and (2) "Sex outnumbered of women."

In the same way it can be shown that every word and every

word group in a question tends to produce a response due to that word or word-group *per se*. Consider for example the responses from pupils in grades 7 and 8 to Test *M*.

M

However certain it may seem to be that men work only because they must, and would avoid labor except for the food, clothing and luxuries that are its rewards, the facts may well be to the contrary. It can hardly be the case that men dislike work because they wish to be utterly idle. For mere rest, mere inactivity, is not commonly enjoyed. To have nothing to do is not what men seek. Were that so, we should envy the prisoner shut up in his cell. If men had to choose between a life spent at eight hours of work daily in a factory and a life spent at eight hours of sitting on a throne without moving hand or foot, many of them would, after trying both, choose the former. Activity of body or mind, at which a man can succeed, is, in and of itself, rather enjoyed than disliked.

1. What is it that this paragraph says may seem sure, but probably is false?

.....

2. In what respect is a prisoner in his cell like a man with a million dollars?

.....

3. If the absence of any activity were what we wished for, what would be our attitude toward a prisoner in his cell?

.....

4. What is stated in the paragraph to be really liked and not objected to?

.....

5. What choice is described in the paragraph as an argument that work, merely as such, is not always avoided?

.....

In question 1 the influence of *Paragraph* is seen in such responses as:

"Work,"
"Labor,"
"Idle men and working men,"
"Idleness and wealth," and
"Men that do not work."

The influence of *Paragraph says* is seen in:

"It says that men should not avoid labor,"
"It says that some men work and some of them would be idle," and
"Some men envy prisoners in a cell rather than work."

The influence of *Sure* is seen in:

"For mere rest mere inactivity is not enjoyed."

Similar evidence in the case of questions 2, 3, 4 and 5 is as follows:

QUESTION 2

prisoner in his cell—"Because he is shut up in a cell," and "A man in prison is sitting 8 hours daily with chains."

QUESTION 3

activity—"Is rather enjoyed than disliked," "For mere rest, mere inactivity is not enjoyed," and "A man succeed or rather enjoyed than disliked."

absence of activity—"Sluggish," "To have nothing to do is what men seek," and "Idleness."

prisoner in his cell—"He would not be there unless do something wrong," and "Because it is not a good thing to be a prisoner."

QUESTION 4

is stated—"Men who work," and "That the American man wants to work."

really liked—"Good clothing and luxuries," and "A hard work man should be liked."

objected to—"Mere rest mere inactivity is not commonly enjoyed," "Inactivity," and "To have nothing to do."

QUESTION 5

choice—"The man who works and the one who does not."

described—"Why men work."

avoided—"They would avoid labor if it was for food."

work avoided—"Idle and stealing."

Consider also the following responses to questions 1, 2, 3 and 4 on paragraph *J*.

J

In Franklin, attendance upon school is required of every child between the ages of seven and fourteen on every day when school is in session unless the child is so ill as to be unable to go to school, or some person in his house is ill with a contagious disease, or the roads are impassable.

1. What is the general topic of the paragraph?

.....

2. On what day would a ten-year-old girl not be expected to attend school?

.....

3. Between what years is attendance upon school compulsory in Franklin?

.....

4. How many causes are stated which make absence excusable?

.....

5. What kind of illness may permit a boy to stay away from school, even though he is not sick himself?

.....

6. What condition in a pupil would justify his non-attendance?

.....

7. At what age may a boy leave school to go to work in Franklin?

.....

1. "Every paragraph must have a period" } due to *paragraph.*
   "A group of complete sentences" }
2. "Monday" }
   "Wednesday" } due to *day.*
   "Friday" }
   "The ten year old girl will be 5a," due to *ten-year-old girl.*
3. "It was a great inventor" }
   "Because its a great invention" } due to *Franklin.*
   "Because it is a small city," due to *Franklin.*
4. Twenty five }
   about ninety } due to *How many.*
   2000 }
   A boy should bring a note }
   If you bring a note } due to *absence excusable.*

In the illustrations given so far the action of the element has been accompanied usually by some vague action of the situation as a whole, but cases may be found where this reduces to about as near zero as is possible, provided the pupil writes any answer whatever.

Consider, for example, the following response to questions 1, 2, and 5 on paragraph *I* (on page 220).

QUESTION 1

"Most of them were old," which shows almost no effect of anything save the "*persons.*"

"There were a lot of floats," which shows almost no influence save of "*parade.*"

"Irish," which could fit any paragraph or any question provided *parade* or *persons* or both occurred therein, almost as well as it fits this.

QUESTION 2

"The chief commander of all" and "Captain and lieutenant" seem to show no influence save the *major* of majority plus a faint effect of *parade.*

QUESTION 5

"1492" and "1776" seem the products of *date* in total neglect of all else in the question and paragraph.

Some of the cases above, if taken alone, are perhaps as explainable by other causes as by the tendency of each word to act irrespective of the total of which it is an element, but no one, I think, will assume that the collection can be well explained save by supposing that the single words do have that tendency. We must then think of the pupil who examines the paragraph and the questions as beset by a tendency to answer each question by each word in the paragraph and have each word in the questions evoke a response that is bound to it alone. Most of these tendencies are of so nearly zero strength that they almost never compete successfully with other tendencies; and most of the resulting thoughts are so absurd that if they come to mind they are promptly dismissed. But a sound theory must accept their existence, traces of which we have illustrated in the case of Paragraphs *I*, *J* and *M*. Every element in a situation tends to arouse the response which is connected with it.

Just as any element of the situation may be, relatively to others, far too potent in determining the response, so also it may be not nearly potent enough. Calling Pa the potency of element a and Pb the potency of element b, Pa/Pb may vary between 0 and ∞ as limits.

The following are some cases of under-potency in the case of *I* (page 220):

QUESTION 1

nearly—(Failure to include this in the response to 1 is of course very, very common.)

fifteen—"Thousand"

what is said about the—"Honorable and good," "They march very nice," "They marched very straight," "They did good or bad," and many similar responses.

number of persons—"They were workers," "Men and women," "That they rejoin in the parade," "Passed before cheering spectators."

who marched in the parade—The many responses of "200000," "They cheered them," etc.

All of question 1 except *parade* is under-potent—"There were a lot of floats."

QUESTION 2

the "out" of outnumbered—"A number of women."

which—"Both sexes was in the parade," "There were both sexes there," "Workers of both sexes," "Men and women," "Two sexes," "Two of them."

sex—"City workers," "City workers of N. Y.," "The chief commander of all," "Working," "The front ones," "Spectators," "Cheering," "Fifteen thousand."

in the majority—"Women," "Sex outnumbered of women."

which . . . was in the majority—"The sex spectators," "Sexes," "In the parade," "Sexes in the parade," "There were men of other sex in the parade."

QUESTION 3

what did the . . . do—"They were cheered by the people," "Two hundred."

people—"Tip his hat."

people who looked at—"Passed before two hundred," "Passed before a number of cheering spectators."

when it passed by—"Two hundred thousand cheering spectators passed."

it—"They saluted them," "They cheered them."

cheering—"Inspected the parade," "They were glad to see it," "They talked about it" (and many others).

All save *parade* underpotent—"September seventh," "Seventh Avenue."

QUESTION 4

saw the parade—"The men outnumbered the women," "Far outnumbered."

two—"About 100000," "One hundred thousand," "Three hundred thousand."

hundred—"Two thousand."

thousand—"Two hundred."

two hundred [thousand]—"Fifteen thousand," "Nearly fifteen thousand," "Over 25000," "Over five hundred," "About 10000," "About 5000," "About 1000."

QUESTION 5

what date—"There were workers of both sexes in the parade," "Thought the man fat out," "Described," "Sexes of the parade," "The parade," "And outnumbered women."

event described in paragraph—"March 4, 1915," "March 17," "April 23, 1903," "November 4," "December 4," "On Friday," "March 17," "March 18," "St. Patrick's day," "On the twenty-second of February," "St. Pattac," "1492," "1776," "1820."

Seventh—"September seventeenth" (a common error, often due to misperception or memory, probably) "September."

In any situation we may distinguish in a rough way between words meaning things, qualities and events on the one hand and words meaning relations between them on the other. Thus we may think of question 2 below as:

[(Use *of* a gas range) *instead of* (use *of* a coal range)]

effect upon

temperature of the kitchen

H

You need a coal range in winter for kitchen warmth and for continuous hot-water supply, but in summer when you want a cool kitchen and less hot water, a gas range is better. The xyz ovens are safe. In the end-ovens there is an extra set of burners for broiling.

1. What two varieties of stoves does the paragraph mention?

.....

2. What effect has the use of a gas range instead of a coal range upon the temperature of the kitchen?

.....

3. What is needed to provide a supply of hot water all day long?

.....

4. For what purpose is the extra set of burners?

.....

5. In what part of the stove are they situated?

.....

6. During what season of the year is a gas range preferable?

.....

These relating elements are subject to over-potency and under-potency like any others, but especially to under-potency. Thus in the question quoted, we have as responses due to under-potency of the *instead of:*

"The stove makes heat" and "You need a coal range in winter and a gas range in summer."

As responses due to under-potency of *effect upon*, we find:

"You can cook better," "Heats quicker," "Because it is summer," and "Because we need no coal."

As responses showing under-potency of both *instead of* and *effect upon*, we have:

"Gas to cook and coal to get warm," and "Gas range in summer."

Elements may act in substantially correct potency but out of their proper relations. Responses to arithmetical problems will illustrate this richly. In reading we have such cases as "A cool kitchen is used for a gas range" in response to *What effect has the use of a gas range instead of a coal range upon the temperature of the kitchen?*

The connections leading from any element or group of elements may be wrong whether the element is under-potent, of correct potency, or over-potent. They may be *wrong in toto* in the sense of leading to unserviceable responses to that element for any purpose, or *wrong for any defined set of purposes*, or *wrong for the test's purpose* in the sense of leading to responses unserviceable for the particular need or problem. Thus "A complete sentence" is wrong for almost all purposes as a response to *paragraph;* "Commas and periods" is perhaps even more universally futile; "A group of sentences" is wrong for the particular purpose of answering the question of *J* 1.

Inadequacy may be considered as a special case of wrongness by lack or insufficient connections, and failure of response as the limiting case of inadequacy.

Incorrect thinking due to wrong or inadequate bonds leading from one or more elements of the situation is a simple consequence of the general facts of connection-forming that does not need demonstration here. We know *a priori* that every element tends to call up what has followed or accompanied it.

I will report only three illustrations. The paragraph being

John had two brothers who were both tall. Their names were Will and Fred. John's sister, who was short, was named Mary. John liked Fred

better than either of the others. All of these children except Will had red hair. He had brown hair.

and the questions being

5. Who had brown hair?.....
6. Who had red hair?.....

there are with pupils of all grades from the third to the eighth a large per cent. number of responses of "John" and "Fred" to "*Who had brown hair?*" and 30 per cent. of responses of "Will" to *Who had red hair?* Under-potency of *All the children except* caused the formation of the connection of *Will* with *red hair*. This wrong connection not only produced the large crop of errors to question 6, but also worked back to prevent the correct answer to question 5.

To question 1 on paragraph *J* we find the following answers, all being more or less wrong connections leading from the element *paragraph*.

"The sentence,"
"A sentence that made sense,"
"Period,"
"Capital,"
"A capital letter,"
"To begin with a capital,"
"Subject and predicate,"
"A letter,"
"In every paragraph must have a period,"
"Capital letter in the first letter,"
"Commas and periods,"
"The paragraph man was marsh."

To the question, on paragraph *T*, *What do you think "heaven's azure" means?* we find the following answers from college freshmen, in whom right bonds leading from *azure* are either absent or so weak as to be suppressed by the wrong (for the purpose) bonds leading from *heaven* or from the general sense of glorified moralizing which the passage establishes. Only about one college freshman in three thinks of so plain a thing as the blue sky!

T

But it is to you, ye Workers, who do already work, and are as grown men, noble and honorable in a sort, that the whole world calls for new work and nobleness. Subdue mutiny, discord, widespread despair, by manfulness, justice, mercy and wisdom. Chaos is dark, deep as Hell; let light be, and there is instead a green flowery world. Oh, it is great, and there is no other greatness. To make some nook of God's creation a little fruitfuller, better more worthy of God; to make some human hearts a little wiser, manfuller, happier,—more blessed, less accursed! It is work for a God. Sooty Hell of mutiny and savagery and despair can, by man's energy, be made a kind of Heaven; cleared of its soot, of its mutiny, of its need to mutiny; the everlasting arch of Heaven's azure overspanning it too, and its cunning mechan-

isms and tall chimney-steeples, as a birth of Heaven; God and all men looking on it well pleased.

"The everlasting rainbow,"
"'Peace,"
"Peace, universal,"
'Peace progress and justice one to another,"
"Peace and the world purged of strife,"
"Peace and happiness love and manliness,"
"The peace and love of man's fellow men,"
"The peacefulness one sees in a God-fearing community,"
"Harmony in life,"
"The millenium,"
"Appreciation of the beauty of nature,"
"Wisdom mercy,"
"Happiness,"
"Enjoyment,"
"The happiness and contentment that comes+" (+ here and later means that more was contained in the response; but nothing correct),
"God's love,"
"God's influence,"
"God's approval of work,"
"God's creation,"
"A world in which +,"
"A place where men +,"
"The light of the world,"
"The light of a new method—of freedom and happiness,"
"Man's outlook on life,"
"High ideal +,"
"The clearing of mutiny,"
"Perfect condition of things with every one happy and working for the best,"
"The work that man has accomplished,"
"Eternity,"
"Eternal life,"
"Eternal blessing,"
"Love your neighbor as yourself +,"
"God is love. Do unto others as ye would have others do unto you."

We must then think of a pupil who answers the questions concerning a paragraph as beset by tendencies of each word and word group in the question to assume undue potency, to become dislocated from its proper relations, and to call up its past accompaniments and sequents. For him to answer rightly means that an elaborate hierarchy of bonds is active and that an intricate set of forces maintains a balance of power. One may become directly aware of at least a part of this complex coordination and subordination of tendencies if he will note just what it implies to respond correctly to this question on paragraph *T: What does the author refer to as a "Sooty Hell of mutiny and savagery and despair"?* One may infer it less directly but more adequately by surveying the hundreds of different tendencies to respond which a paragraph and question evokes in a thousand pupils and realizing that almost or quite all of these tendencies were present as truly in any one successful pupil, but were prevented from determining final response by some organization within themselves or by some guiding tendencies from without.

Sometimes the correct balance or organization reduces to

the simple case of letting one element be as potent as it may and reducing the potency of all other elements to negligible amounts. We then have the case of reasoning of which James has given the classic description. But the task of thought is, it seems, not usually to choose only one element in the situation for potency, or to accept one only of the facts evoked by that element. Usually there are many elements to be let work together and many evoked facts to be used for the purpose at hand. In our illustration, the "workers," "sooty," "mechanisms" and "tall chimney" all need to be given potency to secure the response of "a region of factories," "a manufacturing community," "a factory town disturbed by labor troubles," or the like, and the "God," "chaos," "Hell" and "Heaven" of Carlyle's grandiloquence all need to be somewhat tempered in their tendencies to call up the world, human nature or other things of vast scope and great moral importance.

Elaborate as are the compositions of forces which give thought its final motion and direction, the forces themselves are of simple nature, being elements in situations and connections leading from these elements to responses which use and (in my opinion) satisfying accompaniments have yoked to each element.

Three simple mechanisms—under-potency and over-potency of elements, dislocation or disrelation of elements, and wrongness or inadequacy of connections—seem to be all that are needed to explain errors in thinking. Conversely, proper balance and organization of elements and right bonds therewith seem to explain correct thinking, no matter how elaborate or subtle. Thinking and reasoning do not seem to be in any useful sense opposites of automatism, custom, or habit, but simply the action of habits in cases where the elements of the situation compete and cooperate notably.

It is of course the case that, along with the balanced action of elements, there goes an inspection and validation of them and the ideas or acts they evoke, whereby each succeeding situation is often amended by increasing or reducing the potency of certain of its elements, and whereby certain

futile ideas may be cast away entirely. These welcomings and rejectings, retainings and letting go, are however themselves nothing more than situation-response bonds, where the response is attending to or turning from, cherishing, repeating, saying *no* or *yes* to, or the like. It is also the case that the "set" or adjustment of the organism plays a more striking rôle in reasoning than it does in mere day-dreaming or routine habit-action, but not a different sort of a rôle.

I conclude therefore that the general laws of human behavior which explain why a pupil puts his clothes on or off and eats or leaves uneaten his breakfast explain why he succeeds or fails in making geometrical demonstrations or scientific researches, and that there exists no fundamental physiological contrast between fixed habits and reasoning.

THE SIMILARITY OF BROTHERS AND SISTERS IN MENTAL TRAITS[1]

BY DANIEL STARCH

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In this study, information was sought on two questions: (1) To what extent are adult siblings (children of the same parents) alike in mental characteristics and (2) is the similarity greater in those mental traits which are directly affected by training in school work than it is in those traits which are not directly affected by school work?

Some experimental work has been done on these problems with siblings and twins of various ages below adult life.[2] The purpose of the present study was to investigate the mental resemblance of adult siblings, and with these problems in mind two series of tests were carried out.

I. For measuring mental functions which are directly affected by school work, the following tests were used: Speed and comprehension of reading ability, size of reading vocabulary, speed and quality of hand-writing, and ability in spelling. These tests were made in exact accord with the methods described elsewhere[3] and hence no further details will be given here. Abilities in the four fundamental operations in arithmetic were measured by the Courtis tests, Series *B*.[4] Ability in arithmetical reasoning was measured by the writer's scale ('Educational Measurements,' p. 114).

II. For measuring mental functions which are not directly affected by school work, the following tests were selected: Two perception tests were used: (*a*) The well-known A-test

[1] This investigation was planned by the writer. The tests themselves and the computation of the results were made very carefully by Clara Fuller Taylor.

[2] Thorndike, E. L. 'Measurements of Twins,' *Archiv. of Phil., Psychol., &c.*, No. 1.

Starch, D. 'The Inheritance of Abilities in School Studies,' *School and Soc.*, 2, 608–610.

[3] Starch, D., 'Educational Measurements' (Macmillan), pp. 20, 38, 60, and 89.

[4] Courtis, S. A., 'Manual of Instructions' (Detroit), p. 58.

which determines the number of A's cancelled in one minute, and (*b*) a geometrical form test which determines the number of a certain geometrical figure cancelled in one minute on a page of similar forms. Memory was measured by the number of words that could be recalled after hearing a series of ten monosyllabic nouns read at the rate of one word per second. Motor capacity was tested by the tapping test in which the task consisted in tapping with the right hand, as rapidly as possible for thirty seconds.

Each test was carried out twice on two different days with a group of eighteen pairs of adult siblings who were students in the University of Wisconsin. The ages of these persons ranged from nineteen to thirty-two. The tests were repeated so as to obtain more reliable measurements of the capacities involved than a single test would yield.

The results may best be presented in the accompanying table of coefficients of correlation computed according to the formula

$$r = 1 \frac{6 \text{ sum } d^2}{n(n^2 - 1)}.$$

In arranging the pairs of siblings, the older ones of the different pairs were placed on one side and the corresponding younger members were arranged on the other. The scores made by each person in each test were then tabulated opposite each person's name. The older members of the various pairs were then ranked by themselves in each test and the younger ones were likewise ranked by themselves. On the basis of these ranks the coefficients of correlation were computed and found to be as follows:

Reading—speed	.51
Reading—comprehension	.64
Writing—speed	.72
Writing—quality	.46
Size of reading vocabulary	.07
Spelling	.05
Arithmetical reasoning	.38
Addition—attempts	.71
Addition—rights	.44
Subtraction—attempts	.43
Subtraction—rights	.29

Multiplication—attempts	.37
Multiplication—rights	.25
Division—attempts	.46
Division—rights	.56
Average	.42
Memory	.31
A-Test	.50
Geometrical form test	.07
Tapping	.65
Average	.38
Coefficients based on ranks in all rests combined	.73

Several interesting results appear in this table. (1) The resemblance of siblings is apparently no greater in those mental traits which are directly affected by school work than in those which are not so affected. The average correlation in the former group of tests is .42 and in the latter .38. This seems to indicate that the mental similarities of children of the same parents are due primarily to heredity rather than to similarity of environment since the resemblance is no greater in those traits which are more directly affected by environment.

(2) The resemblance of siblings is approximately as great in mental traits as in physical traits. Pearson found the correlation between brother and brother in height to be .50 and in cephalic index (ratio of length to width of head) .49. These correlations for physical traits are a little larger than the ones found here for mental traits taken separately. The correlation, however, calculated on the basis of a combined rank for each person in all mental tests together was found to be .73. This greater correlation for all tests combined as compared with the correlations for single traits is due partly to the variation of the correlations among the single traits and partly to the imperfections in the separate tests, which are counterbalanced to some extent in a combined ranking.

(3) Apparently the resemblance is greater in some traits than in others. Spelling ability, range of vocabulary, and perception of geometrical forms seem to be correlated very slightly; whereas speed in writing, speed in tapping, and speed in addition seem to be correlated very closely. To what

extent different mental traits are correlated by greater or less amounts can not be stated with confidence on the basis of the present tests. Further measurements are necessary.

The chief significance of the present results consists in further corroborating the notion that the mental make-up of human beings is as much a matter of heredity as their physical make-up and that environment plays a relatively small part in producing the resemblance of closely related individuals.

A METHOD OF RECORDING ERRORS IN FORM BOARD TESTS

BY E. K. STRONG, JR. AND EDWARD P. GILCHRIST

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The length of time required to perform a Form Board Test has been, in most cases, the principal element in measuring the performance. Various efforts have been made to utilize the error-element also. But most of these have not led to anything of real value except to the originator of the scheme, himself. Yet all who have used form board tests have come to realize that they learned as much, if not more, from the way the test was performed as from the time it required. The reason the errors have not been used more has been due to the difficulty of recording them in the few seconds one has at his disposal.

Miss Schmitt[1] has recently very vigorously protested against using time as a measure of efficiency and has substituted in its place an analysis of the 'method of attack.' She reduces the child's method to one of three types: 'planned,' 'trial and error,' and 'chance.' The writers believe that this analysis should lead to some very desirable additions to our present methods of evaluating a child's ability, but they are not prepared as yet, to accept Miss Schmitt's rather arbitrary method of determining what is a 'planned' or 'trial and error' performance. However, her position raises again the great need of devising a method by which one can keep track of the various movements by which a child solves the form board test. The following method has been found useful in this connection and is recorded here as a possible help to others who have been confronted by the same problem.

Suppose we are working with the Healy-Fernald Construction Test *A*. This test can be solved by making five

[1] Schmitt, C., "Standardization of Tests for Defective Children," PSYCHOL. MONOG., No. 83, 1915.

moves and in three seconds (our best record): or it may be solved after a great number of moves and after a long period of time. Ordinarily five or ten minutes is considered sufficient time in which to test the child's capacities. Now, if one uses coördinate paper, illustrated in the plate, he may draw a diagonal line upwards and to the right for every 'placement'; a horizontal line to the right for every 'rearrangement'; a diagonal line downward and to the right for every 'removal.' The record illustrated in the plate tells this following story. Three blocks were picked up, one after the other, and placed in the rectangle: two rearrangement moves followed; one block was removed from the frame;

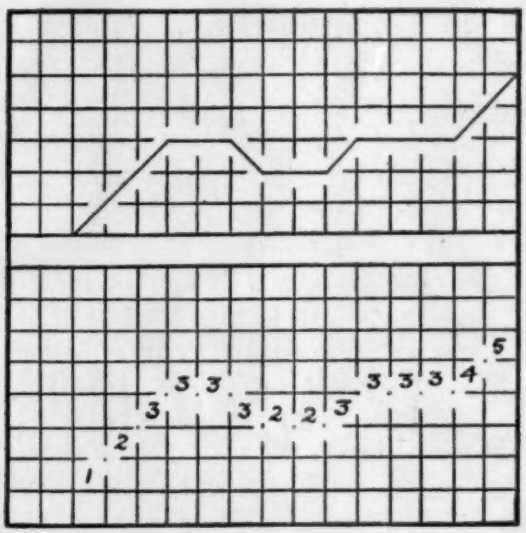

FIG. 1. Showing Method of Recording Movements in Solving Healy-Fernald Construction Test *A*.[1]

two rearrangements of the two remaining blocks followed; another block was placed in the rectangle; three rearrangements followed; the last two blocks were placed in position.

[1] The lower cut should have been so prepared that the two numerals "2" in the center of the cut and the three uumerals "3" near the right of the cut should be placed on their respective horizontal lines instead of above them. If this had been done the distinction between a numeral on a diagonal and one on a horizontal line would appear more clearly to the eye.

This record can be summarized as follows: Placements—6; Removals—1; Rearrangements—7; Total—14.

If one cares to go into further detail in his analysis of the movements, he may write a number from one to five instead of drawing the horizontal or diagonal lines; each number standing for a block. In this way, just what block was involved in the movement may be recorded. Such a record will appear as in the lower part of the figure.

Note: The blocks are numbered in accordance with their length; blocks four and five being, of course, identical. From the record it is clear that block 1 was correctly placed; block 2 was not correctly placed; block 3 was also not correctly placed; it was moved twice and then removed from the rectangle; block 2 was then moved twice, the second time being placed correctly; then block 3 was replaced incorrectly and moved three times, when it was finally correctly placed; then blocks 4 and 5 were placed correctly.

A slight amount of practice enables one to think the correct number when a block is touched; also to write the numbers uphill when they are being placed, downhill when being removed, and on the line when they are being moved about. And all of this can be done while keeping the eyes for the most part upon the subject.

DISCUSSION

INTROSPECTION VERSUS THE SUBCONSCIOUS

Do not the following experiments show that an assumption lies at the base of the conclusions drawn from certain introspective data regarding the mental processes involved:

1. An experienced observer is given a number, say 6, to multiply by 3, and he answers 18, having had no visual nor auditory images nor indeed any thing, so far as he could determine, to give to protocal. But if he is previously told to observe the steps of the process in multiplying 9 by 4 he often reports a visual image $\begin{matrix} 9 \\ 4 \\ \hline 36 \end{matrix}$ and perhaps also auditory and other images in connection with the solution of the problem. That is, in conducting the experiment in the second way the steps of the process appear.

2. Something similar is also observed often when an experienced observer is instructed to reverse the hands of a clock, that is, he has nothing to report except the correct answer until he has been told to observe the steps of the process with greater attention.

3. The same thing came out in a striking way when a student investigating recognition once complained to me that an experienced reagent with whom he was working was evidently a very careless observer as he had little or nothing to give to protocal. After the reagent had been told of the complaint and instructed to observe more carefully the experimenter informed me that this reagent's introspections had become more detailed, as regards the steps of the process.

4. In reaction time experiments the introspections are usually much fuller after the observer has been told to observe what occurred in the Vorperiode, etc., that is, when instructions have been given similar to those given by Ach in his reaction time experiments. That is to say, in the above experiments, the steps of the process were observable only after the reagent had been directed to note them, that is, after they had been drawn from under the threshold of consciousness through the method employed.

As to whether the steps of the process as they appear in the

second mode of conducting the experiments just mentioned are the same as in the first mode, one may suppose, (1) they are or (2) they are not or (3) may have no definite opinion regarding the matter. If one supposes the processes are the same, he is assuming, is he not, that the processes above and below the threshold of consciousness are the same, that is, that the conscious and the subconscious are fundamentally one; if, on the other hand, he rejects this idea altogether he holds the opposite view regarding the relation between the conscious and the subconscious; if he does not know what conclusion to draw from the data obtained by the two different methods he has no definite opinion regarding the relation of the conscious to the subconscious. It would seem, would it not, from the data given that the opinion one holds regarding the relation between the conscious and subconscious has a somewhat greater significance in introspective work than has been generally supposed? In short, that our opinion of the value of some introspective data depends upon our view of the relation between the conscious and the subconscious, in fact, of the subconscious itself.

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THE MNEMONIC FEAT OF THE 'SHASS POLLAK'

Some years ago, through the kindness of my friend Professor Hollander, of the Johns Hopkins University, my attention was directed to a special achievement in memorizing which I venture to report; since, so far as I know, it has remained unnoticed by psychologists, and yet should be stored among the data long and still richly gathering for the study of extraordinary feats of memory.

The facts of the case I can hardly do better than to allow the witnesses themselves to state. And first the Reverend Dr. David Philipson, of Cincinnati, to whom I was first referred by Professor Hollander.

"The Babylonian Talmud" he has been good enough to write me, "consist of twelve large folio volumes comprising thousands of pages. All the printed editions of the Talmud have exactly the same number of pages and the same words on each page. This must be borne in mind in order to understand the remarkable feat of memory about to be described. There have been, as there undoubtedly still are, men who know the whole text of the Talmud by heart. Some years ago one of these men, a native of Poland, was in this country. I witnessed his remarkable feats of memory. Thus, one of us would throw open one of the volumes of the Talmud, say the tractate Berakhot, at page 10; a pin would be placed on a word, let us say, the fourth word in line eight; the memory sharp would then be asked what word is in this same spot on page thirty-eight or page fifty or any other page; the pin would be pressed through the volume until it reached page thirty eight or page fifty or any other page designated; the memory sharp would then mention the word and it was found invariably correct. He had visualized in his brain the whole Talmud; in other words, the pages of the Talmud were photographed on his brain. It was one of the most stupendous feats of memory I have ever witnessed and there was no fake about it. In the company gathered about the table were a number of Talmudic experts who would readily have discovered fraud had there been any. The technical name which was used by the Jews of aforetimes to designate these memory experts was Shass Pollak; Shass is the abbreviation for the Hebrew terms for the Talmud, and Pollak is Pole; nearly all these memory experts

came from Poland; a Shass Pollak then is a Pole who has memorized the entire contents of the Talmud and is able to give exhibitions of his mnemonic powers like those mentioned above."

And next let me quote from Judge Mayer Sulzberger, of Philadelphia, who in answer to my inquiry, wrote as follows:

"I have met but one 'Shass Pollak' in my life. He was brought into my library one evening by a friend. I conversed with him and experimented upon him.

"After he had been introduced as the expert in question I expressed some curiosity with perhaps a mien of incredulity. He was eager for the fray.

"You are of course aware that all (or nearly all) modern editions of the Talmud are paged alike and printed alike, each page beginning and ending with the same word in all the editions.

"I went to the case and took out a volume of the *first* edition which has its own paging *not* followed by the other editions. He made an automatic dive for a word in a particular part of the page, and lo! it was not there.

"Confounded by this unexpected event, he thought at first that this was not a Talmud I was showing him; and when convinced finally that it was, seemed to bear it some resentment for its improper behavior.

"I then brought out the corresponding volume of an ordinary edition and he undoubtedly made good.

"He would take a pencil and merely glancing at the page put it down anywhere and without looking told the word on which his pencil had lighted. This he did over and over again. There is no reasonable ground for the suspicion that he saw the words. I watched him closely and am convinced that he did not. He had, I feel sure, a perfect image of the page and the position of every word on it in his 'head.'"

Finally, let me give the testimony of Dr. Schechter, of New York, the late President of the Jewish Theological Seminary of America—testimony the more interesting in that while it depends upon the recollection of an experience many years ago, yet it is an independent account of the same kind of testing which Dr. Philipson reports —namely, by pricking through the pages—and consequently confirms the opinion of Judge Sulzberger that the success of the 'Shass Pollak' who was tested merely by pencil was not due to a sly catching of the word by eye.

President Schechter stated to me by letter that once he had

come across a 'Shass Pollak' but that it was too long a time ago to give an account of him with definiteness. "It is at least forty-five years since the incident occurred," he wrote. "What I remember was that he could tell you the contents of every page of the Talmud by heart. I remember also that the people amused themselves by prying a needle into any volume of the Talmud, and he could tell exactly the word on which the needle touched. But I also recollect distinctly that it was nothing more than a verbal or rather local memory, the students all maintaining that he knew very little about the meaning of the contents, their interpretation and application. I heard afterwards of many similar 'Shass Pollaks,' but it is a fact that none of them ever attained to any prominence in the scholarly world."

This absence of any scholarly grasp of the contents thus memorized, of which President Schechter speaks, also appears in the judgment of Dr. Philipson. "I looked upon his achievement at the time I witnessed it as purely mechanical," he writes. "It is quite likely that he could not interpret the Talmud though he knew its contents by heart." And Judge Sulzberger, when proposing to his 'Shass Pollak' that he use his knowledge to some scientific or literary end, was listened to with respect, but nevertheless received the impression that such proposals were deemed by his man to be nonsensical.

All of which confirms the oft-repeated observation, that such extraordinary powers of memory may exist in a kind of intellectual disproportion where there is no corresponding development of other powers—where, indeed, there may be an actual stunting of other powers and interests; as though the mind had 'run' to memory, and been enlarged here at the expense of other functions.

As to the more precise amount of matter that was memorized, it should be noted that a page of the Babylonian Talmud consists, as my colleague Dr. Popper, has pointed out to me[1], of the text proper, called the *Gemarah*, and printed as a more central portion on the page, and of a commentary printed below and around this text. Upon special inquiry whether the mnemonic feat applied only to the *Gemarah* or included also the Commentary, Dr. Philipson

[1] Professor Popper has also referred me to the articles "Talmud" and "Mnemonics" in *The Jewish Encyclopedia* for evidence that at one period the Talmud was handed down solely by memory. The feat of the Poles here recounted may therefore be regarded perhaps as the survival of a custom among early Jewish students in many and widely-separated communities. The work of Brüll, *Die Mnemotechnik des Talmuds*, Vienna 1864, should also be cited.

states that the test which he witnessed was upon the *Gemarah* only; and Judge Sulzberger is of the opinion that this was also true in the case that came under his observation. Even so, the task must have been a stupendous one; the amount of reading-matter upon each page is still great, and the number of pages is enormous.

In closing may I express my thanks, in which other students of psychology will certainly unite with me, to the gentlemen who have so generously given the facts above recorded.

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